THURSDAY, OCTOBER 9, 1873

FOREIGN ORDERS OF MERIT

IN a recent number of NATURE (vol. viii. p. 292) we intimated that honours had been conferred upon a large number of British men of science by the Emperor of Brazil and the King of Sweden. Some of the gentlemen to whom these Foreign Orders have been offered have, however, thought it right to refuse acceptance of them, mainly from loyalty to Her Majesty's stringent regulations respecting Foreign Orders, as issued by the Secretary of State for Foreign Affairs. A correspondent, who has himself refused to accept the Foreign Orders alluded to in our note, has favoured us with a copy of these regulations, and as many people are ignorant of their nature, or are even unaware that any such regulations exist, we shall be doing a service by giving them publicity in our columns. These "Regulations respecting Foreign Orders" are dated Foreign Office, May 10, 1855, and are as follows :—

"1. No subject of Her Majesty shall accept a Foreign Order from the Sovereign of any foreign country, or wear the Insignia thereof, without having previously obtained Her Majesty's permission to that effect, signified by a Warrant under her Royal Sign-Manual.

"2. Such permission shall not be granted to any subject of Her Majesty, unless the Foreign Order shall have been conferred in consequence of active and distinguished service before the enemy, either at sea or in the field; or unless he shall have been actually and entirely employed, beyond Her Majesty's dominions, in the service of the Foreign Sovereign by whom the Order is conferred.

"3. The intention of a Foreign Sovereign to confer upon a British subject the insignia of an Order must be notified to Her Majesty's Principal Secretary of State for Foreign Affairs, either through the British Minister accredited to the Court of such Foreign Sovereign, or through his Minister accredited at the Court of Her Majesty.

"4. If the service for which it is proposed to confer the Order has been performed during war, the notification required by the preceding clause must be made not later than two years after the exchange of the ratifications of a Treaty of Peace.

"If the service has been performed in time of peace, the notification must be made within two years after the date of such service.

"5. After such notification shall have been received, Her Majesty's Principal Secretary of State for Foreign Affairs shall, if the case comes within the conditions prescribed by the present regulations, and arises from naval or military services before the enemy, refer it to Her Majesty's Principal Secretary of State for the War Department, previously to taking Her Majesty's pleasure thereupon, in order to ascertain whether there be any objection to Her Majesty's permission being granted.

"A similar reference shall also be made to the Commander-in-Chief if the application relates to an officer in the Army, or to the Lords of the Admiralty if it relates to an officer in the Navy.

"6. When Her Majesty's principal Secretary of State for Foreign Affairs shall have taken the Queen's pleasure on any such application, and shall have obtained Her Majesty's permission for the person in whose favour it has been made to accept the Foreign Order, and wear the Insignia thereof, he shall signify the same to Her Majesty's Principal Secretary of State for the Home Department, in order that he may cause the warrant required by Clause 1 to be prepared for the Royal Sign-Manual.

"When such warrant shall have been signed by the Queen, a notification thereof shall be inserted in the *Gazette*, stating the service for which the Foreign Order has been conferred.

"7. The warrant signifying Her Majesty's permission may, at the request and at the expense of the person who has obtained it, be registered in the College of Arms.

"8. Every such warrant as aforesaid shall contain a clause providing that Her Majesty's licence and permission does not authorise the assumption of any style, appellation, rank, precedence, or privilege appertaining to a knight bachelor of Her Majesty's realms.

"9. When a British subject has received the Royal permission to accept a Foreign Order, he will at any future time be allowed to accept the decoration of a higher class of the same order, to which he may have become eligible by increase of rank in the Foreign Service, or in the service of his own country; or any other distinctive mark of honour strictly consequent upon the acceptance of the original Order, and common to every person upon whom such Order is conferred.

"10. The preceding clause shall not be taken to apply to decorations of the Guelphic Order, which were bestowed on British subjects by Her Majesty's predecessors King George IV. and King William IV., on whose heads the crowns of Great Britain and of Hanover were united.

"Decorations so bestowed cannot properly be considered as rewards granted by a Foreign Sovereign for services rendered according to the purport of Clause 2 of these Regulations. They must be rather considered as personal favours bestowed on British subjects by British Sovereigns, and as having no reference to services rendered to the Foreign Crown of Hanover."

Having given these Regulations, we may be permitted, perhaps, to make some remarks upon them. It will be seen that so far as scientific men, as such, are concerned, they are positively interdicted from accepting Orders offered to them by a foreign sovereign except in the improbable case of their doing scientific work for such a sovereign. On the face of them it is evident that they are the product of a time when it was thought that such rewards gained otherwise than on the field of battle might be open to suspicion. We can well understand that there may be reasons why diplomatists, projectors, and the like are better without such Orders, but these reasons do not apply to men of culture, whom a king might delight to honour for work done for mankind at large.

It is clear, therefore, either that the triumphs of Science and her followers were little known or were unappreciated when these Orders were issued, or that such possible recipients were purposely excluded. But are not the triumphs achieved by scientific men over the multitudinous forces of nature of infinitely more importance to humanity, and far more conducive to the highest glory of any country, than the greatest military triumphs that soldiers have ever achieved? Indeed, to what is it supposed that the dire art of war itself has reached its present state of comparative perfection, if not to the advantage which has been taken of the discoveries of Science? And does not the military superiority of one nation over another depend almost entirely on the thoroughness with which scientific theories have been applied to army organisation and the *matériel* of war?

It seems to us unjust and cruel that men of science, to whose labours it is mainly owing that our country and the world generally are mounting rapidly higher and higher in the scale of civilisation, should be practically debarred from accepting the few honours that come in

their way. Moreover, we should think that those who have the framing of these Regulations ought to be proud to think that our country produces so many men of science whom foreign sovereigns delight to honour, and instead of throwing obstructions in the way, should afford every reasonable facility to those who are thus honoured to accept and wear the Foreign Orders which may be offered to them. We cannot see that in any way their doing so would endanger the safety of the country nor be derogatory to the dignity and honour of our sovereign. May we not hope, then, that these Regulations as to Foreign Orders should not for ever remain as they are? They certainly permit one to infer that the only glory which those who promulgate them desire to see shed upon their country, is the barbarous glory which can be gained by a good fighter.

We shall be glad to receive the opinions of scientific men on this question.

LUBBOCK'S "MONOGRAPH OF THE COLLEMBOLA AND THYSANURA"

Monograph of the Collembola and Thysanura. By Sir John Lubbock, Bart., M.P., &c. Pp. 265. Seventy-eight plates. (Printed for the Ray Society: 1873.)

THE insects which constitute the Linnæan genus *Podura*, though small and apparently insignificant, present many interesting peculiarities of structure, and still more interesting characters bearing on the great problem of the true affinities and historical evolution of insects generally. They have, however, been comparatively neglected, and those who have worked at their classification have often done so in ignorance of each other's labours, so that the nomenclature of the group is confused. Sir John Lubbock has patiently investigated the characters of the British species, and compared them with those given by Gervais, Nicolet, Bourlet, and Tullberg. The genera he has been led to adopt are arranged in a tabular form on page 39. He gives good reasons for separating *Podura*, *Degeeria*, *Sminthurus*, and their allies from *Lepisma* and *Campodea*; and, while retaining Latreille's name Thysanura for the latter group, proposes for the remainder the new term "Collembola" (κόλλα, ἔμβολον), in allusion to the projection by which they attach themselves to foreign bodies. If this be adopted, there will be no title to designate all the insects belonging to Latreille's Thysanura; but though there is some inconvenience in restricting the meaning of a term already in use, the author would probably hold that the distinctions between the two orders are too great for them to retain with advantage a common name. The change would then be very much like what has been made in separating the herbivorous Cetacea of Cuvier from the rest, giving them a new name, and retaining the old one for the remainder. The relative affinities of either group to other Arthropoda are difficult to decide on. The absence of wings has long, and with ample reason, been discarded by entomologists as a character of importance in classification; the absence of tracheæ, though at first sight more important, does not apply to *Sminthurus* (not *Smynthurus*); the mouth is unlike either the mandibulate or the suctorial type; and the caudal appendage and ventral tube are too peculiar to be of service for comparison. On the whole, the author concludes that "if we represent the divisions of the Articulata like the branching of a tree, we must picture the Collembola and Thysanura as separate branches, though small ones, and much more closely connected with the Insecta than with the Crustacea and Arachnida."* After the chapters on the previous literature of Thysanura and their classification and affinities, comes what to many naturalists will be the most interesting part of the book, a discussion on the evolution of Insects, the origin of wings, and the light thrown on these questions by the study of the groups in hand. It would be impossible to do justice to this chapter in the limits of this article, and it is the less necessary since Sir John Lubbock has lately given our readers an exposition of his views on this subject in the series of papers lately published in these columns on the Metamorphosis of Insects. The remainder of the work consists of a general account of the anatomy of the Collembola and Thysanura, in which there are numerous exceedingly valuable original observations, and a systematic description of the characters, habitat, manners and customs of the various genera and species at present known, with copious synonymy. The value of the work is further enhanced by an appendix by Mr. Joseph Beck, on the Scales of Collembola and Thysanura, illustrated by twelve beautiful microscopic drawings, from the hand of the late Mr. Richard Beck. Thus the various points of interest offered by the groups treated of, to the microscopist, the entomologist, and the natural philosopher, are fully illustrated. Beside the figures, most of them coloured, many showing different stages of growth, which illustrate nearly fifty of the species described in the text, there are numerous careful outlines of anatomical details, which supply what is too often neglected by systematic naturalists. The tribute paid by the author to the artist whose intelligent skill has overcome the most grievous obstacles, will be endorsed by all who see these beautiful drawings.

We congratulate the Ray Society on the production of so excellent a work. This and the preceding volume by Prof. Allman on the Gymnoblastic Hydroids, will maintain its reputation, and we trust that a society to which we owe such works as Darwin's "Cirripedia," Parker's "Shouldergirdle," and Huxley's "Oceanic Hydrozoa," will continue to make so good a choice of books to publish, and will be still more widely supported than it is.

P. S.

MONCKHOVEN'S "PHOTOGRAPHY"

Traité General de Photographie. Sixième Edition. Par Dr. v. Monckhoven. Avec figures dans le texte et trois planches photographiques. (Paris, 1873. Georges Masson, Libraire-Editeur, Place de l'Ecole de Médecine.)

THE great advance made by photography as an art, and the yearly increasing number of processes, have made it almost an impossibility for anyone [not professionally engaged as a photographer to keep abreast of the tide of improvement.

* The relation of both to the Myriopoda is expressed in a sentence which some error of the press has rendered unintelligible. It would seem to make the Collembola, alone, a group of equal "value" with Myriopoda. We may remark here that there are an unusual number of misprints.

It is therefore with great pleasure that we welcome Dr. v. Monckhoven's "Traité Général," which seems to omit nothing in the way of recent additions to the number of photographic processes.

The Doctor commences his book with an historical notice of the origin of the art, in the course of which the irrepressible Egyptians make their appearance as having undoubtedly observed the effects of light on certain bodies; but, unfortunately, they have not handed their experience in the matter to posterity. The Egyptians and Greeks, however, having been disposed of, we have sixteen pages of really very useful historical matter, so arranged that a short paragraph is devoted to each of the more important processes, and which is rendered still more valuable by numerous references to the original papers of the various investigators to whom we owe the art.

The author then proceeds to give a sketch of the nature of light. Perhaps in a treatise of this sort one cannot expect a very comprehensive definition of such a subject. Still, however, something more satisfactory than the following might have been expected . . . . "il existe nécessairement entre le soleil et nous, un certain mode de communication dont nos yeux sont l'intermédiare; c'est ce mode de communication qui constitue ce que l'on appelle la *lumière*."

We then have a sketch of the chemical action of light, and a very good description of what a photographic laboratory ought to be, but, we fear, very rarely is. Considerable space is devoted to a description of the method of preparing the various substances required, including gun-cotton and collodion; and here we may observe that Dr. van Monckhoven makes use of the old system of chemical equivalents obsolete in England, and very nearly so on the Continent, a proceeding which is to be regretted in a work which is likely to remain for some time a standard book on its subject. We have noticed that photographers are singularly conservative on this point, for, to the best of our belief, there is not even now a photographic journal which makes use of the present atomic system of notation, a system which even nine years ago was largely used by chemists. A really admirable chapter on photographic optics succeeds that on photographic chemistry; one soon perceives how much the art has owed to the lenses constructed on the formulæ of Dallmeyer and Steinheil, and to the credit of English opticians we find that in the summary the lenses of the former are stated to surpass all others.

After dealing with cameras, printing frames, studios, and every other photographic requisite, the various processes are dealt with at length. Here we may note that specimens are given of two of the more recent mechanical printing processes, the "Woodburytype," and "Heliotype." Both are pigment methods, and so are not liable to the slow fading inevitable to the ordinary prints containing silver. Of them we can only say that while it is difficult to imagine that any process can surpass the former for artistic effect, the latter seems equally unsurpassable for any purpose requiring excessively minute and faithful reproduction of fine detail, such as is required in copying maps, prints, or diagrams.

A specimen of what is modestly termed the "retouche des clichés," is also given, but here we feel that we are treading on dangerous ground, as a portrait of a lady is the subject. Suffice it to say, that the general effect of this process seems to be like that of the elixir vitæ, and to make the happy patient young and handsome again.

We find considerable information also on photographic enamelling, and on the production of enlargements, where we observe that the heliostat and its use are described.

The work is illustrated with 280 woodcuts, executed in a style which is only found in foreign scientific works, and three specimen photographs are also given. In conclusion we must congratulate Dr. van Monckhoven on the production of so useful a book, hoping only that the chemical portion will be modernised and extended in future editions. Why do not some of our many amateur or professional photographers devote some attention to the chemical nature of their art? Of the rationale of many of the reactions we know absolutely nothing, and of the others our knowledge is not much greater. Such a research would not be of theoretical value only, but would materially aid in the attainment of that perfect application of means to ends by which alone the best results either in art or science can be obtained.

OUR BOOK SHELF

The Relations of the Air to the Clothes we wear, the Houses we live in, and the Soil we dwell on. Three popular lectures delivered before the Albert Society at Dresden. By Dr. Max von Pettenkofer, Professor of Hygiene at the University of Munich, &c. Abridged and translated by Augustus Hess, M.D., Member of the Royal College of Physicians, London, &c. (London: Trübner and Co., 1873.)

Dr. Hess has done well in translating these lectures by so great an authority on hygiene as Dr. Pettenkofer. Though the author does not believe that any knowledge of real value can be imparted by means of popular lectures, still they serve a good purpose in the way of "scientific edification and elevation, which are to raise our minds and hearts and to affect us like listening to good music." Though we in this country have perhaps less need to be instructed in the rules of hygiene than the mass of people on the Continent, still, it will be universally admitted that very few are acquainted with the principles which underlie healthy living, and still fewer can be at the trouble to put them into practice. In the little volume before us, which is well translated by Dr. Hess, the author expounds in an interesting and yet thoroughly scientific manner, the *rationale* of healthy living so far as our relations to the air are concerned, and shows the scientific principles on which we should choose our clothes both as to material and make, and which should guide us in building our houses. In the third lecture he speaks of the relations of the air to the soil, or on the Ground-air, and shows how much remains to be done before the principles of hygiene and their practical application can reach anything like perfection. The following extracts will give an idea of Dr. Pettenkofer's method of treatment:—

With regard to Clothing, the author says:—"When exposed to *luminous heat*, the *materials* of our clothing do not show very great differences, but in experimenting on shirtings of different *colours*, the following result was obtained:—When white absorbed 100, pale straw colour absorbed 102, dark yellow 140, light green 155, dark green 168, Turkish red 165, light blue 198, black 208. In the shade these differences nearly vanish. Krieger, in experimenting on tin cylinders filled with warm water, has found that a double tight covering by the same material does not retard the heat loss much more than a single one; but when the outer layer was

loose it retarded it very much. From this follows the practical truth, that we can produce a very different effect by the same number of clothes according to their make.

"Generally our clothing has been considered as an apparatus for keeping the air from us. This conception is utterly erroneous, and we can bear no garment which does not allow of a continual ventilation of our surface. Just those textures which are most permeable to the air keep us warmest. I have examined different materials for their permeability to air, and taking the permeability of air passing through flannel as 100, linen allowed 58, silk 40, buckskin 58, chamois 51, kid 1 part of air to pass through them. If the above-stated notion were correct, kid would keep us 100 times, chamois warmer by half, than flannel, and so on, while everyone knows that it is quite the reverse."

With reference to Fur the author says:—"A fur is so arranged that its fine hair projecting into the air intercepts all the heat which flows from the surface of the body by radiation and conduction, and distributes this heat through the air which circulates between the single hair-cylinders. Thus the air, however cold it may be, reaches the nerves of our skin as a warmed air. Furred animals in winter, when touched superficially, give a very cold sensation; it is only near the skin that their hair feels warm. In a severe cold, certainly little of our animal heat comes as far as the points of the hair, from which it would escape by radiation or conduction, as the current of air in the fur cools the hair from its points towards its roots, and a severe cold penetrates only a little farther into the fur, without reaching the skin of the same. This can take place only at an exceedingly low temperature, or when a very cold air is in violent motion. In a well-furred animal the changes of temperature in the surrounding air only change the latitudes at the cold and warm zones in the fur; the place where the temperature of the body and the air equalise each other, moves between the roots and points of the hair, and for this reason a furred animal is not warmer in summer than in winter. In summer its heat leaves at the points, in winter near the roots of the hair."

Journal of the Proceedings and Annual Report of the Winchester and Hampshire Scientific and Literary Society, vol. i., part ii. 1871-2 (Winchester: Warren and Son, 1873).

We are glad to see from the Third Annual Report of this Society that it continues prosperous, the number of members being, in 1872, 105. We hope good use will be made of the valuable herbarium of flowering plants, ferns, lichens, &c., collected and arranged by the late Mr. Hill, which has come into the possession of the society, through the generosity of the Mayor, Mr. R. P. Forder, and the President. The present part of the journal contains a number of papers, literary and scientific, read at various meetings of the society. The principal one is the Introductory Address delivered at the commencement of the third session, by the Rev. Canon Kingsley, on "Bio-Geology—the science which treats of the distribution of plants and animals over the globe, and the causes of that distribution." The address is an eloquent one, it can easily be imagined, shows extensive knowledge and great shrewdness, and contains many valuable hints both to young and old naturalists. Most of the other papers are also by clergymen, the principal ones being the following:—"On the Dawn of Thought in Greece," by the Rev. W. Awdry; "On the Metamorphosis of Lepidoptera," by Mr. J. Pamplin; "the Planet Jupiter," by the Rev. E. Firmstone, in which the author gives many interesting facts and speculations as to the condition of that planet; "Vesuvius previous to and during the Eruption of 1872," by the Rev. C. A. Johns, in which the author describes an ascent he made shortly before the last eruption, and appends a condensed abstract of Palmieri's account of the eruption. Appended to the journal is a valuable list of 315 works on the Geology, Mineralogy, and Palæontology of the Hampshire Basin, compiled by Mr. William Whitaker, of the Geological Survey.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Wyville Thomson and the Ventriculidæ

I TRUST that you will afford me a little space for a few remarks upon some passages in Prof. Wyville-Thomson's book, the "Depths of the Sea," which, owing to many engagements, has only just come into my hands. So earnest a labourer in the wide field of truth will not, I hope, deem me discourteous if I point out one corner where his feet have slipped; and if it be objected that, after all, it is only in a small spot, the learned Professor will, I am sure, agree with the answer that, even in the smallest steps towards truth, attainable accuracy is important.

In 1847-48 my father published a series of papers in the "Annals and Magazine of Natural History," which were afterwards collected into a volume, on the "Ventriculidæ of the chalk, their microscopic structure, affinities, and classification." This work, which still remains, I believe, the authority on its subject, introduced order and classification where before all was confusion, expressly founding these upon two guiding principles of anatomy, the existence of which had been proved by searching tests. These two principles—the first being the structure, the second the fold, of the membrane—I am careful to recall, as I think there is considerable misapprehension regarding them. The chief locality of these fossils was in the south and west of England.

In his chapter on the Continuity of the Chalk, Prof. Thomson brings forward several families of ancient fauna as palæolontological evidence in support of his argument. Among these he devotes some attention to the Ventriculidæ (he calls them *Ventriculites*, but why? In the same sentence he uses the family name *Hexactinellidæ*); but, though he acknowledges my father's work, and refers to his "minute and most accurate description of their structure," it does not appear by what follows that he has quite comprehended it: "He (Mr. Toulmin Smith) found them to consist of tubes of extreme tenuity, delicately meshed, and having between them interspaces usually with very regular cubial or octohedral forms" ["Depths," &c., p. 482]. This description (the Professor will forgive me for saying so) does not convey a very clear idea of any structure, and certainly does not apply to the Ventriculidæ: if the word "tube" here means the body of the creature, it may in one sense be partially true of a few species in each of the genera—*Ventriculites*, *Cephalites*, and *Brachiolites;* but if it is intended to apply to the substance of the structure, I must say that it denotes a complete error. My father's words are, that "the membrane of the Ventriculidæ is composed of very delicate fibres," "the fibre is single and solid, never fistular," and that in this structure "*there are no tubes whatever*" (pp. 21, 25, 30). My father carefully describes this membrane, and marks it as the essential characteristic of the whole family of Ventriculidæ. Among the thirty-five species, for the most part marked by strong differences, he points out that *Ventriculites simplex* is the type of the whole family, consisting of a single membrane without a trace of fold.

Now, Prof. Thomson gives a figure of the octohedral structure to which I will not take exception, but he writes underneath it, "*Ventriculites simplex*, Toulmin Smith. Section of the outer wall, showing the structure of the silicious net-work." This implies, while citing my father's name (1) that this structure is proper to that species; and (2) that there is an inner wall. It also speaks of the net-work as silicious, while, two pages before, it is said, that "Mr. Toulmin Smith supposed that the skeleton of the Ventriculite had been originally calcareous." But though mistakes of this sort might easily arise through misapprehension, I must say I was very much astonished to see the figures, one of the entire fossil, the other of the "outer surface," given as "*Ventriculites simplex, Toulmin Smith*," from Mr. Sanderson's collection ("Depths," &c., pp. 483, 484.) A glance at Fig. 1, on the second plate in my father's book, will show that the name

has been misapplied to this specimen, which, as far as can be judged from the drawing, appears to be either *Ventriculites quincuncialis*, or one of the *Cephalites*, both quite different in outward appearance from the plain *simplex*. I know that it is often not so easy to distinguish the species of those preserved in flint as of those in chalk, but in this instance it is quite evident that it is not *simplex*.

My object in writing the above has been to vindicate my father's scientific accuracy, and to recall the facts he worked out. With regard to another point: it is stated by Prof. Thomson that some of the beautiful sponges discovered in the late deep-sea dredgings, especially the *Holtenia* and its allies, and the Ventriculites, "belong to the same family, in some cases to very nearly allied genera," or, as Dr. Carpenter puts it ("Good Words," October 1872, p. 703):—"Here we found the type of the old Ventriculites, which were supposed to be extinct, still living on in the deep sea." Much as my father would have delighted in the exquisite beauty of these new forms (the *Euplectella* he had examined in 1848), I do not think that he could have acknowledged the *Holtenia* as belonging to the ancient Ventriculidæ; nor, if the use of the word "type" depend for its force upon the character of structure, can it be truly said to be a type of that family. True, it possesses a silicious skeleton, but so does the Euplectella; and neither from Prof. Thomson's description ("Depths," pp. 70–72), nor my own examination, can I discover in the Holtenia any trace of or resemblance to the delicate structure and folded membrane of the Ventriculidæ. With great deference, therefore, to the opinion of these investigators (if I am wrong I will gladly learn), it appears to me that the modern type of the old Ventriculite is yet to be found.

I will add that the series of specimens figured in my father's book is in the British Museum, open to examination by students, together with a large portion of hi collection of the Ventriculidæ.

Highgate, Sept. 27 LUCY TOULMIN SMITH

"Deidamia"

I NOTICE in Prof. Wyville Thomson's extremely interesting papers the name *Deidamia* v. Willemoes-Suhm, used for a crustacean genus. This name must be changed, inasmuch as it is preoccupied in Articulata by Dr. Clemens in 1859. Dr. Clemens has used the title for a valid genus of North American Sphingidæ. I propose, therefore, for the genus in Crustacea, the name *Willemoesia*, in honour of its discoverer, with the two species *leptodactyla* and *crucifer*, the former the type.

AUG. R. GROTE,
Curator of Articulata, B.S.N.S.

Buffalo, U. S., Sept. 15.

Dr. Sanderson's Experiments and Archebiosis

IN a communication made to the British Association during its recent meeting at Bradford, Dr. Sanderson criticises the experiments of Prof. Huizinga, and also throws doubt upon the validity of the conclusions which I have drawn from experiments of my own. The "Note" appears in your columns this week; and seeing the nature of the conclusion drawn by Dr. Sanderson from his experiments, I am not a little surprised to find no mention in it of one most important point, viz., the temperature at which Bacteria are killed when immersed in fluids.

It must be obvious to all who understand the real nature of the question at issue, that no valid conclusion can be drawn by Dr. Sanderson from his experiments, unless he is able to argue from a definite conviction as to the temperature at which Bacteria are killed in fluids.

Now a study of Dr. Sanderson's writings would show the reader that up to the time of their publication he had every reason to believe that Bacteria were uniformly killed in fluids at a temperature of 100° C. If he still believes this to be true, he cannot (in the light of facts which he has learned concerning the productivity of previously boiled fluids in closed flasks) refuse his assent to my main proposition, viz., that Bacteria are capable of arising in fluids independently of living reproductive or germinal particles.

But the conclusion which Dr. Sanderson does draw from his experiments, and his imputation that facts do not warrant the conclusion of Prof. Huizinga and myself, would seem to imply that he is in possession of some new evidence subversive of his previous opinion, and tending to contradict views which I have recently published concerning the death-point of Bacteria in heated fluids. ("Proceedings of Royal Society," Nos. 143 and 145, 1873.)

As Dr. Sanderson is entirely silent upon this point, I venture to ask, both for my own information and for that of your readers, whether he still believes that Bacteria are killed by a temperature of 100° C. in fluids; and if not, upon what grounds he has changed his opinion?

In the face of his expressed intention (not a little contradicted, as I venture to think, by his public action) of taking no part in the "spontaneous generation" controversy, I ask Dr. Sanderson this question, because I cannot suppose that he would publicly throw doubt upon the validity of the conclusion which Prof. Huizinga and I have drawn from our experiments, in the absence of fresh evidence of his own upon the thermal death-point of Bacteria.

At present he has publicly expressed the opinion that we are not warranted in our conclusions, whilst he has given no sufficient information either to the world of science or to ourselves by which to test the correctness of his own conclusion. This seems neither just to us nor to himself.

H. CHARLTON BASTIAN

University College, Oct. 3

Mr. D. Forbes's Criticism of Mr. R. Mallet's Volcanic Theory

AFTER the lapse of half a year Mr. D. Forbes has recurred in NATURE for Sept. 4, 1873, to my remarks published in NATURE of March 20 last, to his remarks upon my Theory of Volcanic Energy and Heat contained in his review of my translation of Palmieri's "Incendio Vesuviano," which appeared in NATURE of February 6 preceding.

I pray your permission to make some remarks upon Mr. Forbes's last production. They are the last by which I shall prolong this unpleasant controversy.

Mr. Forbes affirmed that if anything was certain, it was that the ejecta of volcanoes in all ages and all over the world are *identical* chemically or mineralogically, and upon this assumption passes a summary condemnation upon my theory, which he predicts will never receive acceptance from anyone—chemist, or mineralogist, or geologist. This rash and I will now say discourteous prediction I at once disposed of by giving the names of two authorities, whose competence even Mr. Forbes could not question, who had already accepted my views.

To this Mr. Forbes now says, that, as these gentlemen possessed for their guidance in assenting to the bare statement of my views, no better information than that upon which he dissented from them, so they may have been mistaken and not he. How is Mr. Forbes sure they had no better information, and can it be possible that he is so dull in weighing the force of evidence as to see no difference in probability of error between two assumed equally competent men—one of whom can assent to a proposition upon his prior knowledge and without waiting for proof; and another, who dissents, before he has heard what can be advanced in favour of the proposition and against his own previous knowledge or supposed knowledge? This, however, is now immaterial except as an indication of Mr. Forbes's capacity for weighing evidence.

To Mr. Forbes's grand objection I replied that it is based upon error as to fact—that it is not true that all volcanic solid ejecta are identical at all times and everywhere.

While I denied, and do again deny, that identity, chemical or mineralogical, exists in those bodies, I admitted that they do present a great general resemblance—which is just what we should expect.

I added a very important remark, namely that whether it were true or false that all volcanic ejecta were identical, chemically or mineralogically—the fact, whether one way or the other, did not apply to or affect my theoretic views as to the nature and origin of volcanic energy and heat; one way or the other, the identity or dissimilarity between the ejecta as found at the surface must be the same, whether they be derived from materials already and constantly in fusion, or be fused by elevation of temperature locally and temporarily produced; the materials fused being the same in both cases.

This last objection, which is fatal to Mr. Forbes's criticism, whether the foundation on which he has rested it be true or false, he either has not noticed or finds it convenient now to ignore.

I illustrated the want of identity, chemical or mineralogical, and yet the great general similarity at all times and places of

volcanic ejecta, by the analogy of the blast furnace, in which the same materials in the same proportions do not even in any one furnace, or at all times, produce identical slags.

What is Mr. Forbes's reply? That the *intention* of the iron master is to produce slags always the same, as the indication that the furnace is working well.

Doubtless the intention and desire of the iron-master is to produce good iron, and at all times as nearly as he can such a slag as indicates that he is doing so. But, as a matter of fact, he is not able to reach this. He can only approximate to constancy in the chemical or mineralogical constitution of his slags, which are never identical, even for short periods. Is this substitution of the intentions of the iron-master for the actual facts of the blast furnace slags, on Mr. Forbes's part, worthy of the candour of the searcher for truth; or does it not rather resemble the dialectic wriggle of the advocate?

Complete identity between any two rocky masses, ejected or otherwise, can only exist where the same elements in the same proportions are combined in the same way, and in the same molecular aggregation. If the mere presence in greater or less proportion in the mass, of certain crystallised minerals in any variable proportion, such as felspar, pyroxene, or leucite, in the magma of lavas, were enough to constitute identity, then nearly all the known rocks of the world, crystalline, igneous, and sedimentary, might be viewed as identical, for all consist of a few elements and of a few prevailing simpler minerals.

While still seeming to maintain his original statement, Mr. Forbes now substitutes for *identity*—a great *similarity* in all volcanic rocks. Further discussion is therefore needless—nor indeed would discussion of my views as to volcanic heat, &c., lead to any good result—with a gentleman whose notions of scientific method are such, that after six months' consideration he holds any distinction between hypothesis and theory to be mere hair-splitting, and whose notions of physico-mechanics are of that confused character, that he views pressure and work to be quite the same, and that it is matter of indifference whether we talk of "pressure converted into its equivalent, heat," or of work transformed into heat.

Would Mr. Forbes enlighten your readers by stating in figures what is the equivalent in heat, of the pressure of a weight of ten pounds, resting upon a rigid level plane?

Were Mr. Forbes of any real authority upon volcanic subjects, there might have been more ground for his sweeping and anticipatory condemnation of my views as to volcanic energy, which, however, in that case, he would never have uttered; but on looking down the list of his published papers, I do not find any treating of vulcanology simply, nor am I aware that he has ever enlarged the boundaries of our knowledge in that department by a hair's breadth.

Mr. Forbes appears to think that chemists, mineralogists, and geologists are the sole arbiters of all questions as to the nature and origin of volcanic heat and energy. Whatever they may have done to add to our knowledge of the visible and tangible phenomena of volcanic vents or cones, they have as yet contributed really nothing to discovering the nature and origin of volcanic heat itself, if we except some valuable negative evidence drawn from the gaseous emanations by chemists of late years, subversive of the older theories of the chemical origin of volcanic heat, still not quite extinct. It is much more to the physicist and theoretic mechanician dealing largely with the *physique du globe*, that we must look for further light, and whose province it will be to decide when the right key shall have been found to that enigma of ages, the true nature and origin of volcanic heat and energy.

I am done, sir, with this controversy, unwillingly entered upon, not in irritation, as Mr. Forbes states, but because I felt justified in protesting against new and I believe important views being obscured *in limine*, by objection based only on error.

My paper containing those views will ere long be before the world. My 100 separate copies (as author) from the "Phil. Trans." are already in the hands of or on the way to many men of science. The volume itself of the "Transactions" will no doubt appear before the end of the year, and to the verdict of the real men of science of the world, versed in the subject and competent to judge of it, I leave the result.

London, Oct. 6 ROBERT MALLET

On the Equilibrium of Temperature of a Gaseous Column subject to Gravity

FROM Mr. Clerk-Maxwell's reply to my note on this subject which appeared in your columns a short time since, it would appear that he does not profess so much fully to explain the difficulty suggested by me as to show that it is capable of explanation, referring your readers to his other works for further information. I would not, therefore, have troubled you further on the subject had it not occurred to me on reading Mr. Maxwell's letter that I could state the case in such a way as to render clearly apparent the grounds for taking different views on this point.

Let a vertical column of gas, subject to gravity and in a state of equilibrium as to pressure and temperature, be divided by a horizontal plane P into two parts, A above and B below.

In the time Δt let a mass M_1 of particles pass in their free course from A to B, and a mass M_2 from B to A.

Let the portion of A from which the particles composing M_1 proceed be called the upper stratum, and the corresponding part of B the lower stratum, then the following consequences may be deduced:—

1. From the equilibrium of density

$$M_1 = M_2$$

2. From the equilibrium of temperature the amounts of work in M_1 and M_2 while passing through P are equal.

3. From the effect of gravity the work in M while in A reckoning from the commencement of the free course of each particle composing M_1, is less than at P, while that in M_2 is greater.

4. Whence it follows that of the two equal masses M_1 and M_2 in the upper and lower strata respectively M_1 contains less work than M_2.

5. The work in M_1 while in the upper stratum reckoned as before, is the same as that of any other equal average mass in that stratum, and the same is the case also of M_2.

6. The average amounts of work in equal masses in the two strata, and the consequent temperatures of the strata are unequal, the lower stratum having the higher temperature.

I suppose Mr. Maxwell would deny the truth of statement (5). I presume he would argue as follows:—

"Of all the particles in the lower stratum which in the time Δt have at the commencement of their free course a velocity and direction such as would take them through P, gravity in selecting those which compose M_2 excludes those whose velocities are insufficient to overcome the effects of their weights, while in forming M_1 particles of low velocity are selected (included?), which, but for the effects of gravity, would not have cut P in their free courses, consequently the particles in M_1 have an average velocity less than that of the upper stratum from which they come, while the particles of M_2 have a greater average velocity than that of the lower stratum, and consequently the inequality of the average velocity of the particles in the two strata cannot be inferred from the inequality of the average velocities of the particles composing M_1 and M_2 while in those strata."

This argument, therefore, assumes the theory that in a given mass of uniform temperature there are particles moving with every velocity from nothing upwards to a certain limit, and mixed in certain proportions. That this is actually Mr. Maxwell's view I own I might have remembered, but I suppose I overlooked it from an impression in my own mind that the molecular motion was to be regarded as being of a planetary (or in the case of gases a cometary) nature. That in masses of the same temperature velocities were to be regarded as practically uniform, except in so far as affected by the distance of the particles apart, and that the so-called impacts of particles were more properly to be regarded as perihelion passages of bodies moving among each other in hyperbolic orbits. If this view is the more accurate one, then obviously the argument which I have assumed that Mr. Maxwell would use, falls to the ground.

Is there no possibility of testing the nature of the thermal equilibrium of a column of still air? The result would at any rate throw an unexpected light on the nature of molecular motion.

Graaff Reinet College, July 19 F. GUTHRIE

The Sphygmograph

DR. GALABIN, in his letter published in your last number, criticises my explanation of the cause of the small wave in the first part of the sphygmograph trace, which he calls the tidal wave. In his criticism he does not take into consideration the hæmodromograph traces of Chauveau, on which my explanation

is entirely based, and without a reasonable interpretation of which no explanation can be considered satisfactory. The hæmodromograph trace proves that the "tidal wave" of Dr. Galabin has a shock origin, as I have shown in the "Journal of Anatomy and Physiology" (Nov. 1872), and that the dicrotic wave is its resulting tidal wave.

Dr. Galabin appeals to the "tidal wave" in the trace from the artery at the foot, in proof of his explanation; I have taken many from that locality, and find that the tidal wave is never represented at all (as my explanation requires), for it is thrown so far back that it becomes blended with the primary rise.

My explanation of the details of the cardiograph is questioned, because my tracings are said to have been taken with "a lever moving on a pivot, and balanced between two springs." Such was undoubtedly the case in my cardio-sphygmograph observations, but not in my paper on the cardiograph trace, when the instrument employed was, what Dr. Galabin recommends, the ordinary sphygmograph, applied to the chest-wall.

As long as Dr. Galabin has not full faith in the reliability of the sphygmograph and its indications, it is almost impossible to maintain an argument with him, for it is hardly worth discussing points which may be only the results of instrumental imperfections. These are now understood, and can be easily eliminated.

A. H. GARROD

Venomous Caterpillars

THE caterpillars mentioned by R. Benson in your paper of August 14, are not at all uncommon in Calcutta. One day my little girl was brought to me with what appeared to be a good sized hairy caterpillar under her arm, and crying as if in pain, and on my trying to remove it in a hurried way, I discovered that it was nothing but a mass of small hairs. The child had put her arm into an empty tub on the inner edge of which the caterpillar was crawling. As soon as she pressed it, she started as if she had been stung. All the servants crowded round the child and pointed to their heads, but as I was not a proficient in their language I could not make out what they meant. I tried to do what I could with my fingers to remove the hairs, but this seemed very painful, and the swelling round about kept increasing. The ayah, however, soon appeared, attracted by the child's crying, and seemed to know what was to be done. She got some of my hair, made a kind of small brush of it, and gently passed it over the injured part. In a few moments the hairs were all removed, and nothing was left but a white blister. This remained for two or three days and then subsided. In the Calcutta schools the boys call these caterpillars "woolly bears," and if stung by them ask for "a head," and a few rubs soon removes the disagreeable appendages.

C. H. C. B.

Calcutta, Sept. 9.

Harmonic Echoes

LORD RAYLEIGH'S notes on Harmonic Echoes recall to my recollection a little experience which I had in hearing what I supposed to be overtones reflected.

I have frequent occasion to cross a portion of an open public park in which there are few trees. When any sharp sounds are heard in the neighbourhood, as, for instance, the sound of the rod in the beating of carpets in a field near at hand, curious responses to the blows of the rod are heard, and these responses or echoes have not the same pitch as the originating sound. I was puzzled for some time to account for this echo in an open park, with almost nothing above the level of the grass but the iron railings, till I satisfied myself, by occupying various positions, that the echoes were reflections of sound from these narrow fences. But why the difference in pitch between the originating sound and the echo? This, I concluded, *might* result from the overtones of the sound being reflected from the thin iron bars which constitute the railing. It was also observable that it was only the sharp sound emitted by the beating rod which was echoed, and not the dull sound arising from the carpet when struck. The hands struck sharply together will also cause an echo from the fences, which is higher in pitch than the sound of the clapping hands. It would be very interesting to experiment on this point by sounding, at a proper distance, notes of known pitch before narrow, upright, or horizontal bars, and then ascertaining the pitch of the echo, and the relation of the latter to the size of the reflecting surface.

W. J. M.

Glasgow

IT appears tolerably well established that harmonic echoes are selective echoes; that is to say, echoes which, from whatever cause, select and return one of the harmonies of the original without the fundamental.

It may perhaps be found that there are other selective echoes than the harmonic kind. In one of the galleries of the very large parish church of Monkstown, co. Dublin, the sound of S is heard with peculiar intensity, both in the singing and in the responses. This is not an echo, but it may perhaps be a fact of the same kind with selective echoes.

Old Forge, Dunmurry JOSEPH JOHN MURPHY

Carbon Battery Plates

COULD you oblige me with information (or state where it could be obtained) respecting the process of manufacture of hard carbon battery plates, as I have some experiments on hand which necessitate the manufacture of plates of a peculiar shape, and I can neither get them made nor obtain sufficient information to enable me to make them well.

Warrington T. W. FLETCHER

Brilliant Meteor

ON the evening of September 7, at about 9.7 P.M., while walking in a northerly direction in one of the streets of Tiverton, I saw a very large and brilliant meteor slowly descend from east to west, but in an almost vertical direction. The sky was almost entirely covered with a thin veil of cloud, which obscured the stars, so that I was not able to note its course with reference to them; but the altitude of the point at which it first appeared was about 45°, its path was inclined to the vertical at an angle of about 5°, and it disappeared behind a roof at an elevation of about 20°, at a point about 90° to the north of the moon which could be seen through the clouds. The light of the meteor was greenish and flickering, and far exceeded in intensity that of Venus when at her maximum brilliancy, but I could not see any train.

T. PERKINS

Reading School

NORTHERN LIMIT OF PHANEROGAMIC VEGETATION

CAPTAIN MARKHAM has most kindly presented to the Herbarium of the Royal Gardens, Kew, a small but very interesting collection of plants brought back by him from his recent Arctic voyage. Amongst them are four specimens which he obtained from Dr. Bessel, who collected them in lat. 82° N., the most northern position from which any phanerogamic vegetation has hitherto been procured. The locality appears to have been on the east side of Smith's Sound. The species are *Draba alpina*, L.; *Cerastium alpinum*, L.; *Taraxacum Dens-leonis*, Desf. var.; *Poa flexuosa*, Wahl.

JOS. D. HOOKER

THE WEALDEN BORING

THE readers of NATURE will be interested in learning that the lowest beds now reached by the Sussex boring are not Wealden, but of marine origin; that the most distinct of the shells yet examined by me is a Lingula, that it is *Lingula ovalis*, a shell of the Kimmeridge clay. The specimens which contain it were placed in my hands by Mr. Peyton, with Mr. Willett's consent. We are, in fact, already below the Wealden, in the pelagic sea-bed far from its ancient shore.

J. PHILLIPS

THE NEW MARINE ANIMAL FROM WASHINGTON TERRITORY

AT the meeting of the British Association in 1872, I exhibited before Section D specimens of some long white bodies resembling peeled willow-wands, which I had received from Barraud's Inlet, Washington Territory, with the information that they were the "backbones of a fish." Subsequently I published what intelligence I

could collect upon the subject in this journal,* and urged the expediency of further investigation in order to discover the true nature of these curious objects. I also called the attention of various correspondents in America to the same subject, and sent them copies of the article in NATURE.

It appears that the problem has now been satisfactorily solved, and that Prof. Kölliker, Mr. Mosely, and other naturalists, who held that these organisms were the axes of an unknown Alcyonarian polyp of the family Pennatulidæ were correct.

In a paper communicated to the Californian Academy of Sciences on the 18th of August last, of which I have received a separate copy, Mr. R. E. C. Stearns states that a specimen of the Polyp, of which these bodies are the axes, had been presented to the Academy by Dr. James Blake. Mr. Stearns describes the polyp at full length, and proposes to call it *Verrillia blakei*. He describes the general aspect of the species as resembling that of *Pavonaria quadrangularis*, but states that the polyps are arranged in "two unilateral longitudinal series."

I may add, that a communication from Dr. Edward L. Moss on the same subject, has been received by the Zoological Society of London, and will be read at one of the meetings next session. P. L. SCLATER

THE RAY SOCIETY†

THE Council, in presenting their thirtieth Annual Report, congratulate the members upon the continued prosperity of the Society.

The lapse of time, so marked by the production of a long series of volumes on zoology and botany, issued under the auspices of the Society, has scarcely lessened the original dimensions of the Printed List of Monographs in preparation and in progress; the completion of old memoirs being ever counterbalanced by offers of works from new authors. A recent proposal by Mr. G. B. Buckton to describe the British Aphides is a case in point. This addition will occupy the place left void by the publication of Sir John Lubbock's very valuable and interesting contribution to the study of insect life.

Since the last annual meeting some attempt has been made, not unsuccessfully, to reduce the arrears in the issue of the volumes. The monograph for the year 1871, the "Collembola and Thysanura," by Sir John Lubbock, Bart., M.P., has already been distributed to the members; the work for the year 1872, the "British Annelids," Part I., containing the Nemerteans, by Dr. W. C. McIntosh, has been so far finished that it will be ready in a few weeks' time for the binder; whilst the volume for the year 1873, the "Spongiadæ," vol. iii., by Dr. Bowerbank, is, with the exception of a single plate, completed.

The Council have considered that it would be to the advantage of the Society if members could obtain the past annual volumes at the original (or in some cases at less than the original) subscription price. With this view resolutions have been passed: first, that the annual volumes, or sets of annual volumes, issued during the last ten years should be purchasable by members at the subscription price of one guinea; and, secondly, that the books in stock, published earlier than the year 1863, should be supplied at a lower cost than that named in previous reports; and, thirdly, that certain of the volumes belonging to the years 1865, 1866, 1867, and 1868, formerly not distributed separately, should be offered to members for sums less than that of the year's subscription.

In accordance with these resolutions, a list of books and prices has been prepared. The volumes may be obtained on application to the secretary.

The volumes in preparation for future years are:—

Mr. St. George Mivart's "Monograph of the Tailed Amphibia."

Rev. O. P. Cambridge's supplementary volume on "British Spiders."

Messrs. Douglas and Scott's work on the "British Hemiptera Homoptera."

Dr. Gaertner's work on "Hybridism in Plants" (Bastarderzeugung), translated from the German by W. Carruthers, F.R.S.

Prof. Haeckel's "Morphologie." A new edition, revised by himself, and translated from the German.

Mr. Hancock's Monograph of the "British Tunicata."

Mr. Andrew Murray's work on the "Coniferæ."

Rev. H. B. Tristram's "Synopsis of the Fauna and Flora of Palestine."

Prof. Westwood's Monograph of the "Mantidæ," with illustrations by Mr. E. A. Smith.

Mr. Buckton's Monograph on the "British Aphides."

The Council, in conclusion, would urge the members to assist in the work of obtaining new subscribers, seeing that very many old friends are being removed from the list of the Society year by year through death and various causes.

ON THE INTERNAL NOSE OF THE PECCARIES AND PIGS

IN examining the sections of the skulls of the Wild Boar the Babirussa, the Phacochoer, and the Peccary, I was struck with the great difference in the form and development of the internal part of the organ of smelling of the peccary as distinguished between it and the other genera.

The Wild Boar, Babirussa, and Phacochoer, have the nasal cavities on each side of the head large, broad, and continued from the outer to the internal nostrils in a simple manner, and they are only separated from the palate by a thin bone, as they are in the sheep and the generality of allied animals. In these animals the turbinal bone arises from the centre of the outside of each nasal cavity, and is divided above into two plates which are rolled backwards, towards the outer side of the nose. There is a perforation between the hinder edge of the intermaxillary bone and the palatine bone in front of the palate behind the cutting teeth which opens directly into the front of the nasal cavity just within the nostrils, as figured in Huxley's "Elementary Atlas," t. i. 4 *d*.

In the peccary the internal nostrils open into a small cavity, which soon becomes tubular, pervading a large hollow cellular part which occupies the space above the palatine bones, and then gives off a large opening on the outer side to the turbinal bones, and is continued in a smaller tube to a small opening on each side of the front part of the palate, behind the cutting tooth. This aperture is evidently analogous to the large perforation in front of the palate of the pigs, but is quite of a different structure. There is a cavity further in near the external nostrils, which forms an opening to the pituitary convolutions, to which I see nothing like in the skull of the pigs. The naso-turbinal is fixed by its upper edge to the upper part of the nasal cavity, and is rolled inwards, and there is a lamina on the lower side from the expanded part of the tubular internal nostril, which meets the one from the upper edge. The whole structure of this part is quite different from that in the pigs, and Phacochoer, and justifies the separation of the Peccaries as a different group from the pigs. I may also remark that in this genus there is a well-marked bony plate on each side of the brain cavity, that separates the edge of the cerebrum from the cerebellum. This septum is only slightly marked in the skull of the wild boar, and is entirely absent in the Babirussa and Phacochoer.

J. E. GRAY

* See NATURE vol. vi. p. 436.
† Extracted from the Report.

ON THE SCIENCE OF WEIGHING AND MEASURING, AND THE STANDARDS OF WEIGHT AND MEASURE *

VI.

AT the time when the metric system was originated, the French standards of weights were the series known as the *Pile de Charlemagne*, the unit being the *Livre poids de marc* of 16 *onces*, and double the *poids de marc*. The metric equivalent of the *poid de marc* was subsequently determined to be 244·753 grammes. The *once* was divided into 8 *gros* (or drachms), and the *gros* into 72 *grains*. The old French *Livre* of 9216 French *grains* was therefore equal to 489·506 grammes, and 7554 English troy grains. The French *grain* was thus equal to 0·818 English troy grain. In determining the new unit of metric weight, it was necessary to ascertain the actual value in terms of the existing system of the *livre* and its subdivisions, of the provisional weights used; and from accurately comparing them with the old standards, it was deduced from the ascertained weight of the measured cylinder, that the weight of a cubic decimetre of distilled water at its maximum density, or at 4° C., which was 0·9992072 of the provisional kilogram, was equal to 18827·15 grains of the *poids de marc*. This, accordingly, was definitively adopted as the true weight of the kilogram, the new unit of metric weight.

The determination by the French Commission of the weight of a cubic decimetre of water at its maximum density differs somewhat from later authoritative determinations made in England and other countries, as may be seen from the following tabular statement :—

Date.	Country.	Observer.	Weight of cubic decimetre of distilled water at 4° C.
			Grammes.
1795	France .	Lefevre-Gineau . . .	1000·000
1797 & 1821	England	Shuckburgh and Kater	1000·480
1825	Sweden .	Berzelius, Svanberg, and Akermann . .	1000·296
1830	Austria .	Stampfer	999·653
1841	Russia .	Kupffer	999·989
		Mean	1000·084

But the latest and most carefully executed determination by Kupffer agrees so closely with the French determination, that the actual weight of the primary kilogram may be taken as nearly identical with its theoretical definition, and sufficiently accurate for all practical purposes.

From the provisional brass kilogram, with its error thus ascertained by the French Commission, two new standard kilograms were constructed by Fortin, one of platinum, the other of brass, and each was determined, after numerous comparisons and the requisite corrections, to be of the true weight when weighed in a vacuum. The platinum weight was constituted the primary metric standard kilogram, and is known as the *Kilogramme des Archives*. Its form is that of a cylinder of about 39·4 millimetres in diameter, and 39·7 metres high, having its edges slightly rounded, being similar to that of the English platinum kilogram shown of the actual size in Fig. 12. The density of the *Kilogramme des Archives* has never been precisely determined, as it has been deemed hazardous to weigh it in water from a fear of its not being entirely free from the arsenic used in preparing the platinum, and of dissolving this arsenic, and thus diminishing the weight of the kilogram. Prof. Miller has assumed the volume of the *Kilogramme des Archives* when in its normal temperature of 0° C to be equal to the volume of 48·665 grammes of water at its maximum density, as determined by its cubic measurement, and consequently its density to be 20·5487. Other computations, however, differ slightly from this determination.

The brass kilogram was intended as the commercial standard, for regulating all ordinary metric weights in air, and was deposited at the Ministère de l'Intérieure Paris. One uniform shape is adopted in France for all brass kilograms. They are made in the form of a cylinder surmounted with a knob. The height of the cylinder is equal to its diameter, and the height and diameter of the knobs are equal to one half those of the cylinder. Like the platinum *Kilogramme des Archives*, the brass standard kilogram was never weighed in water, and its volume has been computed from its cubic measurement to be equal to that of 124·590 grammes of water at its maximum density, thus making its density 8·206. In our standard air, $t = 62°$ F. $b = 30$ in., the platinum standard kilogram will thus displace 59·25 milligrams of air, and the brass kilogram 151·75 mgr.; the apparent weight in air of the brass kilogram is consequently about 92 mgr. less than that of the platinum standard. This brass kilogram was assumed by the French Commission to be 88·5 mgr. lighter than the platinum standard, when weighed in ordinary air.

The primary platinum metre and kilogram were presented by the Commission on June 22, 1799 to the Corps Legislatif at Paris, and were legally constituted as the standards of length and weight of the new metric system throughout France by the law of Dec. 9, 1799. They were deposited at the Palais des Archives.

A platinum copy of each of the primary metric standards of the metre and kilogram was constructed at the same time, and deposited at the Paris Observatory. These standards, known as the *Mètre de l'Observatoire*, and the *Kilogramme de l'Observatoire*, were considered as next in authority to the primary standards.

The unit of capacity of the metric system, the *litre*, represents theoretically the measure of volume of a cubic decimetre, or the cubic contents of a metallic vessel of this capacity when at the temperature of melting ice. But practically, there is no material primary standard litre, and the legal measure of the litre is determined from the kilogram; that is to say, the litre actually is a measure containing a kilogram weight of distilled water at its maximum density. Such a measure can only be verified by computation, as the vessel itself must be taken at a different temperature from the water contained in it, the vessel at 0° C., the water at 4° C. Authoritative tables are therefore prepared for ascertaining the allowance to be made in every case for differences of temperature from the normal temperature, as well as for the difference of weight of air displaced by the metallic weight and the larger volume of water.

For metric measures of surface, the *are*, equal to 100 square metres in the unit; and for solid measures, more particularly for measuring wood, the *stere*, or cubic metre, is the unit.

The number and denominations of the metric weights and measures actually used in France and other countries, for which specific standards are provided, are as follows: they include the double and the half of each decimal unit, with a duplicate unit to make up the number 9 units :—

6 Metric Measures of Length . .	Double metre Metre, divided into tenths or decimetres, &c. Half-metre, " " Double decimetre, divided into centimetres and millimetres Decimetre, " " (For land) Chain of double dekametre, or 20 metres, divided into metres, and links of 2 decimetres

* Continued from p. 389.

30 Metric Weights	20, 10, 5, 2, 1, 1 kilograms	
	500, 200, 100, 100 grammes	(hectograms)
	50, 20, 10, 10 „	(dekagrams)
	5, 2, 1, 1 „	
	0·5, 0·2, 0·1, 0·1 gramme	(decigrams)
	0·05, 0·02, 0·01, 0·01 „	(centigrams)
	0·005, 0·002, 0·001, 0·001 gramme	(milligrams)

13 Metric Measures of Capacity	Hectolitre,	or 100	litres
	Demi-hectolitre,	„ 50	„
	Double dekalitre,	„ 20	„
	Dekalitre,	„ 10	„
	Demi-dekalitre,	„ 5	„
	Double litre,	„ 2	„
	Litre,	„ 1	litre
	Demi-litre,	„ 0·5	„
	Double decilitre,	„ 0·2	„
	Decilitre,	„ 0·1	„
	Demi-decilitre,	„ 0·05	„
	Double centilitre,	„ 0·02	„
	Centilitre,	„ 0·01	„

Total number of metric weights and measures used in France and other countries, 49.

For dry commodities, the demi-dekalitre is the smallest measure used. The litre being equal to a cubic decimetre, or 1,000 cubic centimetres, in volume, is also equal to 1,000 grammes weight of distilled water at its maximum density; consequently the

Demilitre	= 500 cubic centimetres, or grammes weight of water.	
Double decilitre	= 200 „	„
Decilitre	= 100 „	„
Demi-decilitre	= 50 „	„
Double centilitre	= 20 „	„
Centilitre	= 10 „	„

There are also graduated measures of 5, 2, and 1 cubic centimetres or grammes weight of water.

The earliest recognition by the British Parliament of the metric system thus established in France took place soon after the close of the war. On March 15, 1816, Mr. Davies brought forward a motion in the House of Commons, which was carried, for comparing the imperial standard yard with the French standard metre. The Government entrusted the necessary operations to the Royal Society, who obtained for the purpose two platinum metres from Paris. These had been verified by M. Arago, by comparison with the French standard. One was an end-standard, like the "Metre des Archives," but was nearly twice as thick, being 7·3 milimetres in thickness.

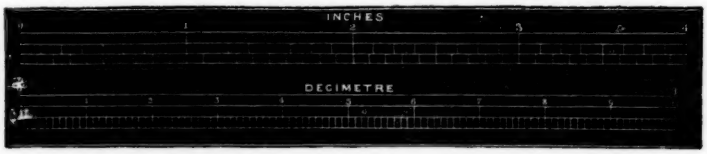

FIG. 11.—Decimetre and its nearly equivalent length of four inches

On one plane surface the word "METRE" is engraved, and on the other "FORTIN A PARIS," and "Royal Society, 44." This end-standard was determined to be exactly the length of a metre at the temperature of melting ice. The other was a line standard, the bar being nearly equal in width, but only 5·3 millimetres thick, and it is about 4 centimetres longer. On the upper surface is engraved "Royal Society, 45," and transverse lines, so fine as hardly to be seen with the naked eye, are cut about 2 centimetres from each end for defining the length of the metre, as shown in the following figure :—

The length of a metre is to be taken between the two transverse lines at the mid-width of the bar, and it has been determined to be less than a metre by 0·01759 millimetre, taken at the standard temperature of melting ice.

On being brought to this country, the two platinum metres were carefully compared by Captain Kater with the length of 39·4 inches on the Shuckburgh scale, considered by him to be the British scientific standard of length. Full details of the comparisons made with Captain Kater's microscopical comparing apparatus are given in Phil. Trans. 1818. It was required to determine the length of the platinum metre at its standard temperature of 32° Fahr. in terms of the brass standard yard of 36 inches at its standard temperature of 62° Fahr. Allowance was made for the different rates of expansion of the two metals, the co-efficient of expansion of the platinum being taken to be 0·00000476 for 1° Fahr., as determined by Borda, and that of brass 0·0000101, as found by Kater's experiments. The length of the metre at 32° Fahr. was thus determined from the *mètre à bouts* to be 39·37086 inches of the Shuckburgh scale at 62° Fahr., and from the *mètre à traits* 39·37081 inches, after allowing for its error = 0·00069 inch. The mean length of the metre was therefore 37·37084 inches of the Shuckburgh scale, and as this scale had been found 0·00005 inch longer than the Parliamentary standard, the true length of the metre was finally determined by Captain Kater to be 39·39079 British inches.

Ever since this period, this authoritative equivalent of the metre in imperial measure has been recognised as the true equivalent, and it received the sanction of Parliament, in the Act of 1864, for legalising contracts made in this country in terms of the metric system. It is, however, to be observed that it is the *scientific* equivalent of the metre in imperial measure. For all *commercial* purposes, on the other hand, the measure of a metre is always used at ordinary temperatures just as a yard measure is used, and the comparison of the two should therefore be more properly made at the same average temperature of 62° F. At such temperature a brass metre is equal to 39·382 inches, and this length is to be taken as the commercial equivalent of the metre in British measure. Of course, this difference of the equivalent in imperial measure of the metre at its legal and at its ordinary temperature, amounting only to $\frac{1}{1000}$ inch is perfectly immaterial in commercial measurements of small quantities, and the metre may safely be estimated as equal to $39\frac{3}{8}$ of our inches, and the decimetre at 3·94 inches, as shown in Fig. 11.

No satisfactory comparison of the primary kilogram with our unit of imperial weight was made until the year 1844, after the construction of the new imperial standard pound, under the authority of the Standards Commission. The comparison of the standard units of weight of the two countries was then undertaken by Prof. Miller, at the request of the Commission. He found that previous determinations of the weight of the kilogram varied amongst themselves from a minimum of 15432·295 gr. to a maximum of 15438·355 grains. Under these circumstances, he proceeded to Paris in the autumn of 1844, and obtained permission from the French Government to compare the Kilogramme des Archives with our English weights. For the comparison, he took with him the Par-

liamentary copies Nos. 1 and 2 of the standard pound, and two auxiliary platinum weights together, equal to about 1432·35 grains. The mean result of 60 comparisons was to find the Kilogramme des Archives epual to 15432·34813 grains. But Prof. Miller was not satisfied with this result, as one of the auxiliary weights was found to contain a small cavity filled with some hygroscopic substance, which rendered its weight slightly variable.

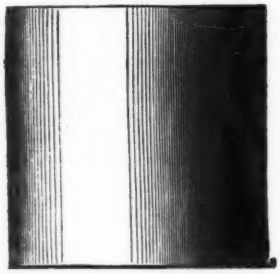

FIG. 12.—Platinum Kilogram 𝔈.

He therefore considered it requisite to make further comparisons directly with the English standard pound.

For this purpose, a platinum kilogram, constructed by Gambey, was procured at Paris by Prof. Miller, and was accurately compared by him with the Kilogramme des Archives. This platinum kilogram, designated as 𝔈 by Prof. Miller, is similar in form to the prototype, but is a little smaller, in consequence of the somewhat greater density of the platinum of which it is composed. Its

FIG. 13.—Gilt Gun-metal Kilogram 𝔅.

density was determined by hydrostatic weighings to be 21·13791. From the mean of 100 direct comparisons with the Kilogramme des Archives, 𝔈 was found to be lighter in a vacuum than the French standard by 1·56 mgr. (0·02412 gr.). For ascertaining the weight of 𝔈 in terms of the new imperial standard pound, Prof. Miller subsequently compared this kilogram with the imperial standard pound, together with each of its Parliamentary copies successively, and one of four auxiliary platinum weights, each of 1432·324 grains, constructed for the purpose, and accurately verified in terms of the imperial standard, by means of supplementary platinum weights. The mean result of 166 direct comparisons of 𝔈 was to find its value = 15432·32462 grains. The Kilogramme des Archives was consequently determined to be equal in a vacuum to 15432·34874 imperial grains, or 2·20462125 standard platinum lb. ; and the imperial standard pound equal to 453·5926525 metric grammes. These equivalents have since been generally accepted, and were legalised in this country by the Metric Act, 1864.

The platinum kilogram 𝔈 has since been deposited in the Standards Department, together with a second kilogram, of gilt gun metal, also made under Prof. Miller's directions, and intended as a standard for the adjustment of commercial metric weights, like the French *kilogramme laiton* deposited at the Ministère de l'Intérieure at Paris. This gilt gun metal kilogram was constructed by Oertling and has been denoted as 𝔄 by Prof. Miller. Its form is spherical with a knob. Its density is 8·3291°. The mean result of 24 comparisons with 𝔈 showed that in a vacuum the weight of 𝔄 was 1·47 mgr. less than 𝔈, and 3·04 mgr. less than the Kilogramme des Archives. In standard air ($t.$ = 18°·7 C., $b.$ = 755·64 mm.) 𝔄 displaced 143·92 mgr. and the Kilogramme des Archives 58·36 mgr. 𝔄 was then found to be 88·6 mgr. lighter in air than the French platinum prototype, and only 0·06 mgr. lighter than the French commercial brass standard kilogram.

Although the metric system was established in France as the legal system of weights and measures in 1799, it was not until more stringent provisions of law for enforcing its exclusive use were passed in 1837, that metric weights and measures began to be generally adopted in that country. Since that period it has been gradually adopted in other countries, and there is now every prospect of its finally becoming universally in use, and being acknowledged as an international system of weights and measures. Attention has been already drawn in NATURE, vol. vii. p. 197, to the proceedings of the International Metric Commission at Paris for the construction of uniform metric standards for all countries who have adopted or contemplated the adoption of the metric system, as well as to the material, an alloy of platinum and iridium, adopted for the new standards, and the peculiar form of the new International standard metre. It will therefore be sufficient here merely to show the adopted form of the new standard metres, as compared with that of the existing Standard Metre des Archives, in the following figures, all of the actual size :

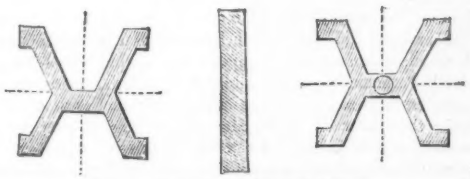

FIG. 14.—Form of New Standard Metres.

The form of the new International kilogram will be the same as that of the Kilogramme des Archives, a cylinder of equal diameter and height, with the edges slightly rounded, as already described.

H. W. CHISHOLM

(*To be continued.*)

NOTES

A LETTER has been addressed by Dr. Anton Dohrn to the Colleges, and other bodies of the University of Oxford, giving an account of the cost, extent, and purposes of his zoological establishment at Naples, pointing out the incalculable advantages

furnished by the establishment to students of biology, and urging that at least one out of the many fellowships belonging to Oxford should be devoted to the purpose of affording a suitable man the opportunity of pursuing the practical study of biology at the Naples station. We have already printed the Report presented to the British Association by M. Dohrn, from which it will be seen that the University of Cambridge has hired a table; we believe the University of Oxford has refused to do so, and hence this appeal to the separate Colleges, by M. Dohrn. Let us hope that at least one of these bodies will come forward and maintain the credit of the University.

PROF. HENRICI's Introductory Address for the session at University College, delivered on Thursday last, dealt chiefly with the distinction between the results of Mathematical teaching in Germany and in England; that while in Germany almost every great mathematician (as an example the late Prof. Clebsch was pointed out) was the founder of a school, in England, on the contrary, no mathematical school had been founded in recent times. This the lecturer did not attribute to the paucity in this country of mathematicians of the very highest eminence,—indeed the names of Sylvester, Cayley, and Sir Wm. Thomson are alone sufficient to show that no country of Europe is ahead of England in this respect,—but rather to the want of personal influence exercised by them on younger minds, which has become almost impossible by the antiquated institutions of our ol d Universities. While the number of mathematical students at Cambridge exceeds that at a large number of German universities put together, the proportion of these students who are pursuing their studies for any higher purpose than that of taking a good degree—after which they allow them to be all but entirely neglected—is very small; and hence England is lamentably deficient in mathematical inquirers of the second and third class. Without wishing to see the German system introduced into this country in its entirety, Prof. Henrici pointed out some of the defects of our English system which he considered to conduce to this end; especially the encouragement given by the mode of examination to "cramming;" the small number of professorships; the fact that the remuneration of the professors is to a great extent dependent on the student's fees, and hence the comparatively high scale of charges; the slight encouragement given to the pursuit of pure science as a means of livelihood; and, above all, the want of that personal communication and interchange of ideas between teacher and pupil which tends so greatly to a promotion of the love of science.

A SPECIAL Meeting of the Council of the Society of Arts was held on Wednesday, Oct. 1, to consider the subject of National Museums and Galleries, and their bearing on public education. A Standing Committee was appointed for the purpose of bringing under Parliamentary responsibility the national museums and galleries, so as to extend their benefits to local museums, and to make them bear on public education. The following are the several objects in view for effecting this purpose:—1. All museums and galleries supported or subsidised by Parliament to be made conducive to the advancement of education and technical instruction to the fullest extent, and be made to extend their advantages to the promotion of original investigations and works in science and art. 2. To extend the benefits of national museums and galleries to local museums of science and art which may desire to be in connection, and to assist them with loans of objects. 3. To induce Parliament to grant sufficient funds to enable such objects to be systematically collected, especially in view of making such loans. 4. For carrying out these objects most efficiently, to cause all national museums and galleries to be placed under the authority of a minister of the Crown, being a member of the Cabinet, with direct responsibility to Parliament; thereby abolishing all unpaid and irresponsible trustees, except those who are trustees under bequests or deeds, who should continue to have the full powers of their trusts, but should not be charged with the expenditure of Parliamentary votes. 5. To enter into correspondence with all existing local museums and the numerous schools of science and art (including schools for music) now formed throughout the United Kingdom, and to publish suggestions for the establishment of local museums. 6. Also to cause the Public Libraries and Museums Act (18 and 19 Vict. c. lxx.) to be enlarged, in order to give local authorities increased powers of acting. We congratulate the Society of Arts on the step it has taken: we believe it is the most important piece of work it has ever set its hands to.

THE Council of the Society for the Promotion of Scientific Industry, looking to the enormous waste there is in the consumption of coal, whilst its cost is every day increasing, have resolved that an exhibition shall be held in Manchester of all appliances and apparatus, that tend to the economic use and saving of fuel, for the purpose of inducing attention to, and eliciting opinions of practical men on the matter, and of giving all consumers of coal an opportunity of comparing the various appliances, with a view to their adoption of that which will best serve their purpose. The exhibition will comprise:—1st. Appliances which may be adapted to existing furnaces, &c., whereby an actual saving is effected in the consumption of fuel. 2nd. Appliances which may be adapted to existing furnaces, &c., whereby waste heat is utilised. 3rd. New steam generators and furnaces, boilers and engines specially adapted for the saving of fuel and appliances, whereby waste products are utilised, and the radiation of heat prevented, &c., &c. The exhibition will include appliances used for manufacturing, agricultural, and domestic purposes. Either the apparatus itself, or diagrams, or models may be exhibited, and no limit is placed upon the class of articles to be exhibited. Exhibitors will be required to deliver their exhibits free of charge at the place of exhibition, and to remove them at the close of the exhibition; they must also erect them if necessary at their own expense. Every exhibit must be accompanied by a full description, which must include a statement of the particular work the apparatus is intended to perform. A duplicate of this statement must be handed in when application is made to exhibit. Exhibitors will be given every opportunity of explaining the speciality of their apparatus. All articles are exhibited at the risk of the exhibitor, though every reasonable care will be exercised. Further information may be obtained from the secretary of the society.

SIR SAMUEL AND LADY BAKER, with their nephew and some black servants, arrived at Paris on Monday morning, *en route* for London. The whole of them are in excellent health, and bear strong traces of exposure to an African sun. Interesting information concerning Sir Samuel's work in Africa, will be found in the *Daily News* of the 7th and 8th inst.

SIR HENRY RAWLINSON delivered the inaugural address on the commencement of the winter session of the Midland Institute at Birmingham on Monday evening. Referring to Arctic explorations, he said he indulged the hope that the year will not close before an assurance has been given that the *Challenger* Expedition will be supplemented by the despatch during next spring of a well-equipped Admiralty vessel which will be commissioned to endeavour to reach the Pole by pushing through Smith's Sound from Baffin's Bay in the track of the American ship, *Polaris*, whose fate has recently elicited so much sympathy throughout England.

SURGEON E. J. MILLIGAN, of the steamship *Africa*, writes from Sierra Leone, on the 12th ult., to the *Irish Times*, stating that on August 17, when returning from Loanda homeward, they steamed up the River Congo, and when at Banana one of

the passengers, M. Cressy, received a letter from a friend stationed 300 miles up the river. It contained the intelligence that about 200 miles farther in the interior a white man, accompanied by a number of native attendants, was proceeding in the direction of the West Coast. His supplies becoming short, he was prevented from proceeding by a tribe, and retained prisoner until some should be secured. From the description given by the native traders to M. Cressy's friend of this person, and also from the fact that no other white man is known to be in this region, it is generally inferred that it is Dr. Livingstone.

WE regret to record the death of Sir Paul Edmund de Strzelecki (perhaps better known as Count de Strzelecki) who died on Monday morning at his residence in Savile Row, at the age of 77 years. Early in life he was a great traveller, and explored a great portion of Australia. He was elected a Fellow of the Royal Society in June, 1853, was a D.C.L., and a member of several of our learned societies.

PROF. WATSON, of Ann Arbor, telegraphs to the Detroit *Tribune:* "On July 24 I observed a star of the twelfth magnitude, which, on Saturday night last (August 16), was missing from the place where first seen. A little to the west I saw a star of the eleventh magnitude, which proves to be the new planet (No. 133), and at present I suppose it to be that seen July 24."

THE Fungus-show at the Royal Horticultural Society on Oct. 1st was a great success; never had there been a greater or better arranged display of these plants, classified under the two sections of "edible" and "poisonous." A new economical use for this class of plants was indicated by the Rev. Mr. Berkeley, who produced a cap made out of the beaten out interior mass of *Polyporus fomentarius*, the amadou or German tinder of commerce, which he described as both warm and light. It is stated that large use is made in Hungary of this material for caps and waistcoats, and it is also used for caulking boots.

ONE of the important and beautiful publications which characterise the Smithsonian Contributions to Knowledge is just issued under the title of "A Contribution to the History of the Freshwater Algæ of North America," by Horatio C. Wood, jun., M.D.

NOW that so much attention is being paid to the introduction into our colonies of useful foreign trees and crops, we desire to call special attention to the publication at Brisbane of "The Olive and its Products: a treatise on the habits, cultivation, and propagation of the tree, and upon the manufacture of oil and other products therefrom," by L. A. Bernays, F.L.S., Vice-President of the Queensland Acclimatisation Society. The work has special reference to the advantages to be derived from the introduction of the olive into Queensland, and is printed and published at the expense of the Colonial Government.

NEWS has been received to the date of May 1, from Mr. Henry Elliott, who has been engaged for two years past in making explorations and observations in the fur-seal islands in the Behring Sea. He announces the continued prosecution of his labours, the results of which were transmitted to the National Museum in the summer of 1872. He has especially devoted himself to an investigation of the habits of the fur-seal, walrus, and sea-lion, and has made a topographical survey of the rookeries upon a portion of the islands on which these animals come to bring forth their young. His work in 1872 was devoted mainly to St. Paul Island, but he expected, very soon after the date of his letter, to visit St. George and the other islands of the group, there to prosecute similar inquiries.

WE consider it extremely creditable to the *Leeds Daily News* that it chronicles regularly and at considerable length the proceedings of the Leeds Naturalists' Field Club and Scientific Association, and we should like to see other provincial, and indeed metropolitan papers follow its example. The principal paper read at the Society's meetings during September was by Mr. James Abbott, on the structure and development of the Hepaticæ. The Society continues, we are glad to see, to investigate very thoroughly the natural history of the district.

WE heartily endorse the following sentiment of the *Athenæum* in reference to the meeting of the British Association:—"The opinion is gradually forcing itself upon many of those who attend the meetings of the Association that some change in its method of procedure is becoming necessary. For the scientific men, on whom rests, more or less, the responsibility of keeping up the sectional business, either by doing official work or attending the meetings and taking part in the discussions, the labour is too exacting on an occasion which should have something of relaxation about it. Again, the tendency of the papers is necessarily to take a technical direction, which must put them beyond the range of the non-scientific audience. The sectional business is consequently unsatisfactory, both to those who take part in it and to those who attend as listeners. The Association should fulfil two functions—first, that of bringing together scattered scientific men, who otherwise rarely or never meet; secondly, of giving the general public some idea of what the scientific world is doing. For the first object, more leisure is required during the meetings—more opportunity of talking over amongst themselves the work which different men are occupied with. To attain the second object, instead of miscellaneous papers, short addresses, carefully prepared, might be delivered, with one or two invited speakers to follow. These addresses should be given at morning meetings, which might advantageously break up at one, leaving the afternoons free.

MESSRS. SAMPSON LOW, MARSTON, and CO. announce the following books to be published during the forthcoming season:—"The Heart of Africa; or, Three Years' Travels, Discoveries, and Adventures in the Unexplored Regions of the Centre of Africa," by Dr. George Schweinfurth. The district explored by Dr. Schweinfurth embraces the wide tract of country extending southward from the Meschera on the Bahr el Ghazal, and between the 10th and 3rd degrees of north latitude. The work will form two large octavo volumes, and will be illustrated by about 130 woodcuts from drawings made by the author during his journey.—"A Whaling Expedition to Baffin's Bay and the Gulf of Boothia. With an account of the rescue by his ship of the survivors of the crew of the *Polaris*," by Captain Markham, with maps and illustrations. The maps to this work will give the first authentic delineation of Hall's discoveries, and also contain several important corrections of the old charts.—"The Land of the White Elephant; or, Lights and Scenes in South-Eastern Asia," being a personal narrative of travel and adventure in Farther India, embracing the countries of Burma, Siam, Cambodia, and Cochin-China, by Frank Vincent, jun., with maps and plans.—"The Wild North Land," a winter journey with dogs across Northern North America, by Captain W. F. Butler, with a map; and a new work on Peru by Thos. J. Hutchinson, F.R.G.S., entitled, "Two Years in Peru, with Exploration of its Antiquities."

MESSRS. TRÜBNER'S List of forthcoming books includes the following scientific works:—"From the Indus to the Tigris:" a narrative of a journey through the countries of Balochistan, Afghanistan, Khorassan, and Iran in 1872; together with a synoptical grammar and vocabulary of the Brahoe language, and a record of the meteorological observations and altitudes

on the march from the Indus to the Tigris, by H. W. Bellew, C.S.I., Surgeon to the Bengal Staff Corps. "The Rod in India;" being hints how to obtain sport, with remarks on the natural history of fish, otters, &c., and illustrations of fish and tackle, by H. S. Thomas, F.L.S., F.Z.S. A third and enlarged edition of the "Celt, the Roman, and the Saxon," a history of the early inhabitants of Britain, down to the conversion of the Anglo Saxons to Christianity, illustrated by the ancient remains brought to light by recent research, by Thomas Wright, M.A., F.S.A.

A DEPUTATION from the Trades' Guild of Learning waited on Tuesday afternoon on a Sub-Committee of the London School Board, at the invitation of the School-Management Committee, in order to urge upon the Board the adoption of systematic training in mechanics, &c., with the object of adapting the scientific instruction, provided or contemplated in the Board schools, to the future employments of the children. A memorial to the same effect has been presented to the Board, and is now under their consideration, in favour of the elementary teaching of applied science and art in the schools, in such a manner as to lay the foundation of a connected system of technical education.

NEWS has been received of the death at Quito, Ecuador, in June last, of Dr. William Jameson, an eminent naturalist, who resided for many years in Quito as a professor of chemistry and botany in the University. His contributions, both in zoology and botany, to public institutions in America and Europe have been very extensive.

DR. DAVID MOORE reprints from the "Proceedings of the Royal Irish Academy" a complete Muscology of Ireland, under the title "Synopsis of all the Mosses known to inhabit Ireland up to the present time."

THE additions to the Zoological Society's Gardens during the past week include two Black-headed Parrots (*Caica melanocephala*) from Demerara, presented by Judge Lovesy; a Brown Bear (*Ursus arctos*), European, presented by Mr. M. B. Wilson; a Thicknee (*Oedicnemus crepitans*), British, presented by Mr. Patey; a Lesser Black-backed Gull (*Larus fuscus*), British, presented by Mr. C. W. Wood; a Hairy Armadillo (*Dasypus villosus*), from River Plate; a Burrowing Owl (*Pholeoptynx cunicularia*), from the same place, deposited; a Wattled Crane (*Grus carunculata*), from Africa, and two Bataleur Eagles (*Helotarsus ecaudatus*).

THE BRITISH ASSOCIATION

SECTIONAL PROCEEDINGS

SECTION A.—MATHEMATICS AND PHYSICS

On Etherial Friction, by Prof. Balfour Stewart, LL.D., F.R.S.

Professor J. C. Maxwell has made a series of experiments on the friction of gases. In these experiments a horizontal disc was made to oscillate in an imperfect vacuum near a similar disc at rest, and it was found that the motion of the oscillating disc was carried away by the residual gas of the vacuum at a rate depending on the chemical character of the gas, and depending also upon its temperature, but nevertheless independent of its density.

While the temperature of the arrangement remained constant, it was found by Prof. Maxwell that this fluid friction was rather greater for atmospheric air than for carbonic acid, while for hydrogen it was, I think, about half as great as for air.

On the other hand, when the temperature was made to vary the result was found to be proportional to the absolute temperature.

These experiments do not show that there is no such thing as etherial friction, that is to say, friction from something which fills all space, and is independent of air; but we may argue from them that such an etherial friction must either have been nearly insensible in these experiments, or it must, as well as the friction from the gas, have varied with the absolute temperature, in which case the two frictions would not be separated from one another by the method of the experiment.

Prof. Tait and myself have made some experiments upon the heating of a disc by rapid rotation in vacuo. In these experiments we found a mere surface heating due to air which varied not only with the quality, but also with the quantity of the residual gas; and we also found a surface effect (more deeply seated however than the former), which appeared to be a residual effect, and which it is possible may be due to etherial friction. We made no experiments at varying temperatures, but we made use of various residual gases, and we found that the heating effect for carbonic acid was perhaps a trifle less than for air, while that for hydrogen appeared to be about four times less than that for air. Now comparing Prof. Maxwell's experiments with ours, we have in the former a stoppage of motion which is rather less for carbonic acid than for air, and about half as large for hydrogen as for air. In the latter again we have a heating effect rather less for carbonic acid than for air, and only about one-fourth as large for hydrogen as for air. Thus it appears that the stopping effect of hydrogen in Prof Maxwell's experiments is relatively greater in comparison with air than is its heating effects in our experiments, when compared with that of air. The effects of these various gases would bear to one another more nearly the same proportion in both experiments, if we might suppose that in Prof. Maxwell's experiments there was mixed up with gaseous friction a very sensible etherial friction; but in that case it would be necessary to suppose that the etherial friction was proportional to the absolute temperature.

During the meeting of the British Association at Edinburgh, I brought before this section reasons for imagining that if we have a body in visible motion in an enclosure of constant temperature, the visible motion of the body will gradually be changed into heat. The nature of the argument was such as to render it probable (although not absolutely certain) that in such a case the rapidity of conversion will be greater the higher the temperature of the enclosure.

I will now refer to some experiments by Prof. Tait which formed the subject of the last Rede Lecture. These experiments were suggested to Prof. Tait by an hypothesis derived from the theory of the dissipation of energy which led him to think that the resistance of a substance to the conduction of electricity, and also of heat, would be found proportional to the absolute temperature. Mattiessen and Von Bose in the case of electricity, and Principal Forbes in the case of heat, had already proved that as a matter of fact the law was not very different from that imagined by Prof. Tait. The result of these experiments has been to confirm the truth of this law.

The following considerations also connected with the dissipation of energy point to the same conclusion. Perhaps we may regard the etherial medium as that medium whose office it is to degrade all directed motion, and ultimately convert it into universally diffused heat, and in virtue of which all the visible differential motion of the universe will ultimately be destroyed by some process analogous to friction.

Now in order to imagine the way in which either may possibly act in bringing about this result, let us imagine some familiar instance of directed motion, as for instance a railway train in motion. The train, let us suppose, and the air in it, are both in rapid motion, while the air outside is at rest. Now as the train proceeds, suppose that a series of cannons loaded with blank cartridges are fired towards the train. A series of violent sounds will go in at the one window, and out at the other of each carriage. Each sound will push some air from the stratum of air at rest into the carriage on the one side, and it will push some air from the carriage into the stratum at rest on the other side. Now in this operation it would seem that part of the visible motion of the train must be taken from it. To make another comparison, it is as if a series of individuals were jumping into the train at the one side, and out of it at the other, the result being that each carries away so much of the motion of the train, and therefore renders it difficult for the engine to drive the train. Each individual comes to the ground with an immense forward impetus, and rubs along the ground till this is lost; in fact, he carries with him so much motion of the train, and converts it into heat by friction against the ground.

Now something similar to this must happen to a substance in visible motion in an enclosure of constant temperature. The rays of light and heat will play very much the same part as the waves of sound, or as the crowd of people in the above illustration, at least if we except those which fall perpendicularly

upon the surface of the moving body. The moving body is like the train, and the rays of light and heat are similar to individuals entering the train from a stratum of ether at rest, and leaving the train into a stratum of ether at rest again, each probably transmuting into heat a certain small portion of the visible motion of the train as it were by a species of friction. Of course the intensity of such an influence would depend upon the intensity of the rays of light and heat. Now it matters not what the particular kind of motion be which constitutes this train—we may assert that all directed motion will suffer from such a cause, and possibly according to the same laws. Visible motion, such as that of a rotating disc, or of a meteor, is of course one form of such motion; but a current of electricity or of heat may equally represent some form of directed motion. In fine, we may perhaps suppose that all forms of directed motion are resisted by this peculiar influence, which evidently depends upon what we may term the temperature of the ether, or at least upon the intensity of those vibrations which the ether transmits.

On a Periodicity of Cyclones and Rainfall in connection with the Sunspot Periodicity, by Charles Meldrum.

At the Brighton meeting (1872) it was stated that the cyclones of the Indian Ocean between the Equator and lat. 25° S., were much more frequent during the maxima than during the minima sunspot years. Since that time the subject has been more fully examined, and I now beg to present a catalogue of all the cyclones known to have occurred during the last twenty-six years. The Tables given last year only contained cyclones of sufficient violence to dismast or otherwise disable vessels at sea, whereas the accompanying Catalogue gives all the cyclones of force 9 to 12, that is, "strong gale" to "hurricane."

The number of cyclones for each year from 1847 to 1873, is as follows:—

	Years.	No.of Hurricanes.	No. of Storms.	No. of Whole Gales	No. of Strong Gales.	Total No. of Cyclones.	No. of Cyclones in Max. and Min. Periods.
	1847	5	0	0	0	5	
Max.	1848	6	2	0	0	8	
	1849	3	2	3	2	10	26
	1850	4	3	1	0	8	
	1851	4	2	1	0	7	
	1852	5	0	3	0	8	
	1853	1	1	5	1	8	
	1854	3	1	0	0	4	
Min.	1855	3	2	0	0	5	
	1856	1	0	2	1	4	13
	1857	2	1	1	0	4	
	1858	3	1	3	2	9	
Max.	1859	3	2	6	4	15	
	1860	7	4	2	0	13	39
	1861	5	2	2	2	11	
	1862	4	2	2	2	10	
	1863	5	2	1	1	9	
	1864	2	2	1	0	5	
	1865	2	2	3	0	7	
Min.	1866	1	4	2	1	8	
	1867	0	4	2	0	6	21
	1868	3	2	2	0	7	
	1869	3	1	3	2	9	
	1870	2	1	5	3	11	
Max.	1871	3	2	3	3	11	
	1872	6	5	1	1	13	36
	1873	4	5	3	0	12	

The observations for the years 1847-1850, are probably not so complete as those for the subsequent years during which the Meteorological Society of Mauritius made it a special duty to collect storm statistics. Still it is evident that not only the years 1860 and 1872, but also the year 1848, were remarkable both for the number and violence of cyclones, while the years 1856 and 1857 were quite the reverse. By taking the number of cyclones in each maximum and minimum sunspot year, and in each year on either side of them, so as to form maxima and minima periods of three years each, we obtain the results given in the last column of the above table, showing that during the maxima periods 1848-1850, and 1859-1861, the number of cyclones was 65, whereas in the minima periods 1855-1857, and 1866-1868, it was only 34, or little more than one half. In 1856, there was only one hurricane of small extent, and in 1867, no hurricane at all. Indeed it is doubtful whether several of the cyclones in those years classed under "storms," should not have been put down in the columns of "whole gales" and "strong gales."

As, during the last twenty-two years, information respecting the hurricanes of the Indian Ocean has been carefully and systematically collected and tabulated, I believe that the results now given are substantially correct, and it seems to me that they point unmistakeably to a close connection betwen sunspots, or solar cyclones, and terrestrial cyclones, or what might be called earth-spots, by an observer, on another planet.

Most of the severest cyclones have already been traced, and the others will also be traced. When this shall have been done, an attempt will be made to express numerically the amount of cyclonic area and cyclonic force for each year. The catalogue gives little more than the number of cyclones, but from what is already known, there is little doubt that their extent and force were also far greater in the maxima than minima years.

Being desirous of extending the investigation as far back as possible I have been examining the lists of former hurricanes, and it is interesting to find that the evidence from this source strongly corroborates the correctness of the conclusions deduced from the observations of the last twenty-six years. From a "chronological table" published in the "Mauritius Almanack" of 1869, we obtain the following list of Mauritius hurricanes:—

Years	No. of hurricanes	Years	No. of hurricanes
1731	1	1818	1
1754	1	1819	2
1760	1	1824	2
1766	1	1828	1
1771	1	1829	1
1772	1	1834	1
1773	1	1836	1
1786	1	1844	1
1806	1	1848	1
1807	2	1850	1
1815	1		
		Total	24

Probably the above list gives only the hurricanes that were remarkable from their destructive effects in the island; and much stress should not be laid on observations taken at a single locality. But it is rather suggestive that out of the twenty-four hurricanes mentioned, seventeen fall within, or very nearly within, maxima sun-spot periods, and only seven within minima periods. Thus:—

Max. Years	No. of hurricanes	Min. Years	No. of hurricanes
1760	1	1731	1
1771		1754	1
1772	3	1766	1
1773		1824	2
1786	1	1834	1
1806		1844	1
1807	3		
1815		Total	7
1818	4		
1819			
1828			
1829	2		
1836	1		
1848			
1850	2		
Total	17		

The same "chronological table" contains the following remarks:— 1760, Dec. 1, "Meteorological Phenomena," 1815, Feb. 5, "Meteorological Phenomena."—I have not ascertained what these phenomena were; but it is not improbable that they were auroras. The aurora of the 4th Feb., 1872, was described in the newspapers as a *phénomène météorologique*.

Baron Grant, in his History of Mauritius, p. 194, regrets the destruction of the woods near Port Louis, because, he says, the town was thereby "exposed to the violence of the winds, as well

as to the heat of the sun;" and in a foot-note it is remarked: "These inconveniences, however, are fully counterbalanced, if it be true that the *cessation of hurricanes since* 1789 has been caused by the great diminution of the woods." As the history was published in or after 1801, it would appear that during the twelve years 1789–1801 no hurricanes occurred. Now, since according to the Table of Sunspots the years 1788 and 1804 were maxima years, and the intervening minimum occurred in 1798, our theory would lead us to expect a comparative cessation of hurricanes during the period mentioned.

If time permitted, I would adduce similar evidence respecting the hurricanes of Bourbon and other parts of the world.

The hurricanes of the Indian Ocean are well known to be attended with torrential rains. So much is this the case that the popular belief at Mauritius is that cyclones are the cause of our rains. Heavy rains over extensive areas are certainly concomitant with cyclones in the Indian Ocean, and it was therefore resolved to examine whether there was a rainfall periodicity. As far as the Mauritius observations went, the matter was clear; but it was desirable to extend the investigation to other localities. The Queensland and South Australia observations, which were the only ones available at the time, gave a similar result, and as Adelaide is far beyond the limits of the region of cyclones, it was surmised that there was a rainfall periodicity generally. The Cape observations afterwards gave additional support to this view. The rainfall of England was next examined, and also found to bear out the hypothesis.

It would occupy much more time than I can at present spare to enter fully into this question of rainfall periodicity. With the help of the researches of Mr. Lockyer, Mr. Symons, and Dr. Jelinek of Vienna, I have now examined 93 tables of rainfall for various parts of the world, and I find that, scarcely without exception, more rain falls in the maxima than in the minima sunspot years. I beg to append a table showing the general results for the quarters of the globe. It will be seen that, so far as the observations go, Europe, Africa, America, and Australia give very favourable results. Asia is only represented by three stations, one of which is Jerusalem, where the excess of rain in one minimum period exceeds the excess in the maxima periods for two stations in India. France is the only European country (of which the rainfall has been examined) that gives an unfavourable return, but it must be remarked that we have as yet got only five stations there, which are all inland, and probably do not fairly represent the rainfall of the whole country.

By taking the longest possible series of observations for several stations, the periodicity comes out, and there is, I think, strong evidence that the rainfall for the whole globe is subject to an annual variation.

Having given the facts, as far as I have been enabled to do so, I abstain from offering any theoretical remarks. If cyclone and rainfall periodicities be fully established, a corresponding (direct) temperature periodicity should exist, and this presumable variation of solar heat may be the indirect cause of the periodicity of auroras and magnetic disturbances.

(The catalogue of cyclones was appended.)

On the Effect of Pressure and Temperature on the Widening of the Lines in the Spectra of Gases, by Arthur Schuster, Ph. D.

One of the questions in Spectrum Analysis yet open to discussion, is what influence pressure and temperature exert on the widening of the lines, which is sometimes observed when an electric current passes through certain gases. The subject of this communication is to point out a little ambiguity which has crept into the very statement of the question at issue, and to show the only way by which a decisive answer can be arrived at, and, in my opinion, has already been arrived at. I shall begin by assuming that the convection of electricity has no direct influence on the character of the spectrum; that is to say that under the same pressure, and at the same temperature, the gas will always show the same spectrum, whether the temperature has been produced by the passage of an electric current or by any other means. In the present state of science this is the only reasonable assumption that can be made, and it has been tacitly made, I think, by every one who has written on the subject.

Let us imagine a vessel filled with hydrogen, and let the temperature of the gas be brought up to incandescence. The heat communicated to the vessel is partly used to increase the translatory motion of the gas, and thereby to increase its pressure, and the other part of the heat has increased the periodical motion in the molecules of the gas, which is generally admitted to be the cause of its incandescence. If the temperature is such that the lines are widened we can account for this fact in two different ways. We may think that the forces which maintain the molecule in vibration, and which are such that at a lower temperature only perfectly isochronous vibrations can take place are somewhat altered, so that the bonds which keep the molecules together are loosened and now allow vibrations to take place, the period of which is somewhat altered and varying. We might secondly explain the widening of the lines by saying that they are caused by the disturbances caused by the frequent shocks of other molecules. If we increase the number or the force of these shocks by increasing either the number of molecules or their velocity we might well obtain disturbances large enough to change a little the period of vibration. These are the only two explanations that can be given, and if we say, therefore, that temperature is the cause of the widening of the lines we can only mean that part of temperature which has its equivalent in the vibrating energy within one molecule. If we say that pressure is the disturbing cause we include that part of heat which increases the pressure with increasing temperature. Let us now see whether we can obtain a clear answer to the question which has now been clearly put.

It is evident that no result can be arrived at by subjecting the same quantity of gas in the same vessel to different temperatures, for we cannot increase the vibrating energy of the molecules without increasing at the same time and in the same proportion (as Clausius has shown), their translatory velocity. By varying in the same ratio the two possible causes we shall never be able to say which is the right view to be taken.

There are two ways open to us to mend this difficulty. We might increase the temperature of the gas *under the same pressure*. If the perturbation caused by the shocks of other molecules cause the widening of the lines this widening ought not to take place as we have reduced the number of these shocks in the same ratio as we have increased their force. If on the contrary the disturbance in the period of vibration has its cause within the individual molecules it ought to remain.

We might, secondly, decide the question in subjecting the gas at the *same temperature* to different pressures. If perturbations are the cause the lines would be widened. Which of these two ways is most easily pursued in experimenting? Can we easily heat up a gas to incandescence under constant pressure? I think not. If an electric discharge takes place in a gas only comparatively few particles of the gas are heated up, and at a very small distance from the points through which the discharge takes place the gas is hardly heated up at all. But if the heat is not diffused through the whole mass of gas, the increase of pressure caused by this gas will also be merely confined to the luminous streak, and we can therefore obtain no answer to our question (as this has been attempted) by filling a tube with a certain quantity of gas, and altering the strength of the passing current or the mode of discharge.

We are, therefore, compelled to abandon this route and to turn our eyes to the second way which I have indicated; but here we meet another difficulty, and even one over which we cannot easily get. We cannot alter the pressure of a gas without altering its electric resistance, and, therefore, also the strength of the electric current and the heat developed. We can only decide the question by subjecting the gas at the *same* temperature to different pressure. Now have there ever been any such experiments made? I think there have, and even very decisive ones. Frankland and Lockyer have found that if we increase the pressure of hydrogen while an electric current is passing through it the lines begin to expand till the spectrum becomes continuous, and finally the resistance becomes so large that the electric current will not pass at all. On the other hand Gassiot and Plucker have observed that if we diminish the pressure of hydrogen its electric resistance force diminishes, attains a minimum, then increases again, and if we keep up exhausting the tube it becomes again so great that the current cannot pass. Plucker says that a tube exhausted to its utmost limits shows the lines of hydrogen and silica. He mentions at one place, I think, that the lines are very fine and distinct. If there would have been any widening he would have been sure to mention it. Now it is not too much to assume that the resistance of the gas at the moment when the discharge just ceases to take place is the same whether the increase of resistance is produced by too great a pressure or too great an exhaustion. At this moment, therefore, the current is the same and the same energy must be converted into heat by resistance. But in the case in which the current does not pass on account of the excessive diminution of pressure, only a much smaller

quantity of gas has to be heated than in the other case. It must, therefore, be heated up to a much higher temperature, and yet the spectrum is not continuous and the lines are not even widened. We are, therefore, compelled to accept Frankland and Lockyer's original conclusion, that pressure and not heat is the cause of the widening of the lines.

The question is one of considerable importance. If temperature would widen the lines, the widening ought always to begin at the same temperature, and the hydrogen in the solar protuberances which show only narrow lines could not be at a higher temperature than the hydrogen in our vacuum tubes, the moment the lines begin to widen. If our conclusion, however, is correct the breadth of the lines will give us no indication whatever as to the temperature of the gas.

Dynamometers, by R. S. Ball, LL.D., F.R.S.

If we adopt that force which acting on one gramme for one second will impart the velocity of one centimetre per second as the unit, then one million of such units is a convenient magnitude for practical purposes. The large figures on the dynamometers represent these million units, for which it is hoped that ere long a suitable name will be adopted. The dynamometers are intended for educational purposes. They are exhibited to the Association with the desire of aiding the present movement in favour of an improved system of fundamental unity.

SECTION C.—GEOLOGY

Concluding Report on *the Maltese Fossil Elephants*, by Dr. A. Leith Adams, F.R.S.

For thirteen years Dr. Leith Adams has prosecuted his researches upon the fossil elephants of Malta, and he now presented the final report upon this subject. Three forms of fossil elephants occur here which are unknown elsewhere, all of small size. The largest is the *Elephas Mnaidriensis* (L. Adams), which attained a height of seven feet. In the crown sculpturing of the molars this species resembles *Elephas antiquus*; as regards the ridge-formula, its nearest ally is *Loxodon meridionalis*. *Elephas Melitensis* (Falconer and Burk) varied in size; its average height was about five feet; this too belonged to the Loxodon group. The smallest bones known to the author belonged to an elephant only three feet high, called *Elephas Falconeri*, by Busk. Although there appears to be some evidence for separating this from the other forms, yet the author stated that "there is no difficulty in arranging a graduating series of specimens from the smallest up to the largest bones ascribable to the *Elephas Melitensis*."

The elephants all occur in the same deposit, and with them there are remains of *Hippopotamus Pentlandi* and *H. minutus*. There is also a gigantic dormouse and a large extinct swan, besides some reptilian remains not yet fully worked out.

The report concludes as follows:—"It must be apparent that this (for the most part) unique fossil fauna restricted to a small mid-ocean island, presents several interesting contrasts with reference to the Mammalia in general, and elephants in particular, which frequented Europe during late geological epochs. For example, between Rome and Sicily we find remains of the *Elephas primigenius*, *Elephas antiquus*, and *Elephas meridionalis*. In the caves of Sicily, traces of the African elephant have been discovered, and also molars, barely distinguishable from those of the Asiatic species, and which, under the name of *Elephas Armeniacus*, are traceable eastward into Asia Minor, in the direction of the present habitat of the living species. It looks, indeed, as if the eastern basin of the Mediterranean had been at one time a common ground where all these extinct and living elephants met, and from whence, with other animals, they have disappeared or been repelled to distant regions."

Sub-Wealden Exploration.—1. *General Report*, by Henry Willett.

In this report Mr. Willett gave a summary of the results achieved up to the present time, the details having already been published in his quarterly reports.

The boring was commenced at the time of the last meeting of the British Association at Brighton, and its object is to explore the rocks underlying the Weald of Sussex. A bore of 6½ inches diameter was at first adopted, but at the urgent recommendation of Mr. Prestwich, one of 9-inch diameter was employed. The bore has now reached a depth of 300 feet, and the engineer (Mr. Bosworth) has contracted to increase it to a depth of 418 feet at the cost of only 1*l.* per foot. Of the 300 feet of strata already passed through, about 70 were previously known, but the remaining 230 are new to science; 50 feet of this consists of valuable beds of gypsum.

Mr. Willett has designed a novel form of drill which possesses the following advantages:—(1) It cuts only the circumference; (2) it makes better progress; (3) the central core is left intact; (4) the tool not unfrequently extracts the core itself. The gypsum was extracted by this means, and it is believed that no such cores have been brought to the surface from similar depths in this country.

Sub-Wealden Exploration.—2. *Geological Report*, by W. Topley, F.G.S.

The author commenced by repeating the protest, often made already, that the Sub-Wealden Exploration was not a "search for coal." It is simply an endeavour to explore the rocks which underlie the Weald and especially to reach the Palæozoic rocks. Whatever these rocks may prove to be, if reached at all, the boring will have succeeded. The results of this boring cannot fail to have important bearings upon the question of the probable occurrence of coal measures beneath the South-East of England, but the discovery of coal is not the object in view.

An account was then given of the lowest beds exposed at the surface in Sussex, and of the reasons which have led many geologists to consider them as representatives of the Purbeck Beds. The thickness of Purbeck Beds previously known in Sussex was somewhat over 300 feet; probably about 230 additional feet of strata have been made known by the boring, in which there are some valuable beds of gypsum.

The boring commences about 250 down in the known Purbeck Beds; up to Sept. 1 it had reached a depth from the surface of 294 feet. It is not safe at present to speculate upon the geological age of the lowest beds reached in our boring, but additional evidence will probably soon be obtained.*

The author then pointed out that most of the bore holes which have been put down to the Palæozoic rock through newer strata have reached those older rocks at about 1,000 feet below the sea. There is a probability then that at or about this depth the palæozoic rocks will occur beneath the Weald. These places, however, are on, or to the north of, the westerly prolongation of the Axis of Artois, whilst the boring is to the south of that line; it is therefore possible that different conditions may prevail here.

Attention was then drawn to the fact, already pointed out by Mr. Godwin-Austen, that the dip of the carboniferous limestone in the Boulonnais is to the south, whilst in the Pays de Bray the same limestone has been found at a depth of 57 feet from the surface, underlying Kimmeridge clay. It is then probable that under the secondary rocks near to the south of Boulogne there is a basin of palæozoic rocks, in which the coal measures may be preserved; this basin might possibly be prolonged to the west below the Wealden district of the south-east of England.

In the course of the discussion which followed the reading of these reports, Sir John Hawkshaw stated that many people, himself included, took an interest in this question chiefly from the hope that coal might be found; but even if in this respect we were doomed to disappointment it would still be of great importance to show that, at that particular spot, no coal existed. Prof. Phillips thought that the object sought was neither coal, gypsum, nor salt; but that *something* exists below the Wealden is certain, and that something we are now searching for. A discussion then took place as to the best mode of conducting deep borings. Mr. R. Russell, C.E., spoke of the great value of the diamond boring process; but from remarks made by other speakers it appeared that, although the diamond is admirably adapted for boring small holes in hard rocks, it is not so well suited for conducting such an operation as that under discussion.

On the Arenig and Llandeilo Rocks of St. David's, by Henry Hicks, F.G.S.

The object of this paper was to follow out the succession of the rocks in the neighbourhood of St. David's, commenced in previous papers communicated to the British Association. The section was now completed to the top of the Llandeilo series. The Arenig and Llandeilo groups were each divided into an upper and a lower series, the author believing that in each case

* Since this Report was read, Prof. Phillips has broken up and carefully examined parts of the cores brought up from the bottom of the boring; in them he has found *Lingula ovalis*, which occurs in the Kimmeridge Clay. (See p. 487.)

there was sufficient evidence to enable him to do so. The Lower Arenig series it was stated occur as black slates and flags about 1,000 ft. in thickness, and are characterised by many species of graptolites as well as by numerous trilobites entirely restricted to the series. The Upper Arenig series occur as fine-grained, soft black shales, not much cleaved, also about 1,000 ft. in thickness, resting conformably on the Lower Arenig series. Their graptolites are distinct from those found in the lower beds, as are also all the other fossils. The Lower Llandeilo series, the lowest rocks recognised by Sir R. I. Murchison in the typical Llandeilo district, occur at St. David's as black slates and hard grey flaggy sandstones, and are about 1,500 ft. in thickness. The most characteristic fossils are *Didymograptus Murchisoni*, *Diplograptus pristis*, *Asaphus tyrannus*, *Calymene Cambrensis*, and *Illænus perovalis*. The Upper Llandeilo series occur as black slates and flags, several thousand feet in thickness, forming several folds of strata, and resting conformably on the Lower Llandeilo series. The typical fossils are *Ogygia Buchii*, *Barrandea Cordayi*, *Calymene duplicata*, *Cheirurus Sedgwickii*, *Trinucleus fimbriatus*, *Ampyx nudus*, and *Lingula Ramsayi*.

The author doubted whether any other spot hitherto examined in Britain could show so continuous a section of these rocks; still he believed that there was ample evidence to prove, from researches made in other parts of Wales and in Shropshire, that the succession here made out was, in most of its important details, capable of being applied to many other districts.

SECTION D.—Biology

Department of Anthropology

On the Relation of Morality to Religion in the Early Stages of Civilisation, by Edward B. Tylor, F.R.S.

Investigations of the culture of the lower races of mankind show morality and religion subsisting under conditions differing remarkably from those of the higher barbaric and civilised nations. Among the rudest tribes a well-marked standard of morality exists, regulating the relations of family and tribal life. There also exists among these tribes some more or less definite religion, always consisting of some animistic doctrine of souls and other spiritual beings, and usually taking in some rudimentary form of worship. But, unlike the higher nations, the lowest races in no way unite their ethics and their theology. As examples, the Australians and Basutos of South Africa were adduced. The Australians believe spiritual beings to swarm throughout the universe; the Basutos are manes-worshippers, considering the spirits of deceased ancestors to influence all the events of human life, wherefore they sacrifice to the spirits of near relatives, that they may use their influence with the older and more powerful spirits higher in the line of ancestry. Yet these races and many others have not reached the theological stage at which man's good or evil moral actions are held to please or displease his divinities, and to be rewarded or punished accordingly. The object of the present paper is to trace the precise steps through which the important change was made which converted the earlier unethical systems of religion into ethical ones. This change appears to have been a gradual coalescence between the originally independent schemes of morality and religion.

In order to show the nature of such coalescence between religion and other branches of culture, not originally or not permanently connected with it, the author traced out on an ethnological line the relations between religion, and on the one hand the rite of marriage, on the other hand the profession of medicine.

First as to marriage:—The evidence of the lower races tends to show that at early stages of civilisation, marriage was a purely civil contract. Its earliest forms are shown among savage tribes in Brazil and elsewhere. The peaceable form appears well in the customs of the marriageable youth leaving a present of fruit, game, &c., at the door of the girl's parents; this is a clear symbolic promise that he will maintain her as a wife. Another plan common in Brazil is for the expectant bridegroom to serve for a time in the family of the bride, till he is considered to have earned her.

The custom of buying the wife comes in at a later period of civilisation, when property suited for trade exists. The hostile form of marriage, that by capture, has also existed among low tribes in Brazil up to modern times, the man simply carrying off by force a damsel of a distant tribe; the antiquity of this "Sabine marriage" in the general history of mankind being shown by its survival in countries such as Ireland and Wales, where within modern times the ceremony of capturing the bride in a mock fight was kept up.

Now in none of these primitive forms of marriage, as retained in savage cultures, did any religious rite or idea whatever enter. It is not till we reach the high savage and barbaric conditions that the coalescence between marriage and religion takes place; as where among the Mongols the priest presides at the marriage feast, consecrates the bridal tent with incense, and places the couple kneeling with their faces to the east to adore the sun, fire, and earth; or, as where among the Aztecs the priest ties together the garments of the bridegroom and bride in sign of union, and the wedded pair pass the time of the marriage festival in religious ceremonies and austerities. So complete in later stages of culture did this coalescence become, that many have come to consider a marriage hardly valid unless celebrated as a religious rite and by a priest.

Second, as to the relation of the profession of medicine to religion. In early animistic philosophy, one principal function of spiritual beings was to account for the phenomena of disease. As normal life was accounted for by the presence of a soul operating through the body, in which it located itself, so abnormal life, including the phenomena of disease, was accounted for in savage and barbaric culture as caused by some intruding spirit. Thus spiritual obsession and possession becomes the recognised theory of disease, and the professional exorciser is the doctor curing disease by religious acts intended to expel or propitiate the demon. Since the middle period of culture, however, this early coalescence has been gradually breaking away, till now in the most civilised nations the craft of healing has become the function of the scientific surgeon or physician, and the belief and ceremonies of the exorcist survive in form rather than in reality.

By these cases it is evident that coalescence between religion and other matters not necessarily connected with it may take place at different periods of culture, and also that this coalescence may terminate after many ages of adhesion. Having shown this, the author proceeded to ascertain exactly when and how in the history of civilisation the coalescence of morality and religion took place.

First, where manes-worship is the main principle of a religion, as among some North American tribes and the Kafirs of South Africa, the keeping up of family relations strongly affects the morality. It is, for instance, a practice among the ruder races to disinter the remains of the dead or to visit the burial place, in order to keep the deceased kinsman informed as to what takes place in his family, in which he is often held to take the liveliest interest. Thus it is evident that any moral act of an individual damaging to his family would be offensive to the ancestral manes, whose influence must therefore strengthen kindly relations among the living members of the tribe. Higher in the social scale this ethical influence of manes-worship takes more definite form, as when in China the divine ancestor of an emperor will reproach him for selfish neglect or cruelty to his nation, and even threaten to induce their own highest divine ancestor to punish him for misdeeds. Thus amongst the ancient Romans, the Lares were powerful deities enforcing the moral conduct of the family, and punishing household crime.

Second, the doctrine of the Future Life begins at the higher levels of savagery to affect morals. In its first stage the doctrine of metempsychosis is seen devoid of moral meaning, men being re-born as men or animals, but when the distinction appears in the higher savagery between migration into vile or noble animals, it is not long before this distinction takes the form of reward or punishment of the good and wicked by their high or low re-incarnation, an idea which is the basis of the Buddhist scheme of retributive moral transmigration through successive bodies. In its earlier stages this doctrine was of mere continuance, as where South-American tribes expected the spirits of the dead to pass to another region where they would live as on earth. Here the distinctions of earthly rank are carried on, the chief's soul remaining a chief, and the plebeian's soul a plebeian, but no sign of moral retribution appears. The first stage of this seems to be where warriors slain in battle are admitted to the paradise of chiefs in the land of the Great Spirit. This idea, which comes into view in several districts, leads to the fuller moral scheme in which goodness of any kind—valour, skill, &c.—are more and more held to determine the difference between the next life of the good man in happy hunting-grounds, or of the bad man in some dismal wilderness or subterranean Hades. In

the higher nations this element becomes more and more distinctly marked, till the expectation of future reward and the fear of future punishment becomes one of the great motives of human life.

Third, when theology among the rudest tribes is mostly confined to consideration of ghosts, demons, and nature-spirits, the intercourse with these leads to little inculcation of moral action. It is when ideas of the great deities become predominant, when men's minds are turned to the beneficent action of the sun, or heaven, or earth, or to a Supreme Deity yet above these, that it is conceived that the order of nature includes moral order of human conduct. Then, as in the religion of ancient China, the universe and its Supreme Deity are regarded as furnishing the model and authority regulating man's actions towards his kindred and his subjects. Thus appears, not in the beginning, but in the middle of the development of religious ideas among mankind, the leading principle of a moral government of the world and its inhabitants.

In these three ways it appears from the evidence of ethnology that the vast transition was made from the earlier unethical to the later ethical systems of religion. Its course, so different from that imagined by the older speculative theologians, has to be ascertained from examination of the actual stages through which the religions of the world have passed. The very attempt to make this investigation on a basis of facts is, however, a novelty.

SCIENTIFIC SERIALS

THE *Monthly Microscopical Journal* commences with an article, illustrated with a plate, "On Organic Bodies in Fire Opal," by Mr. H. J. Slack, in which the author, from the appearances which he finds and describes, expresses an opinion, though not a decided one, that these minute bodies may be vegetable fossils, possibly algæ, though the evidence he adduces is extremely slight. Dr. G. W. Royston Pigott continues his researches on the high-power definition of organic particles, and re-affirms that the generally received description of the Podura scale is erroneous, on account of the employment of spherically over-corrected objectives.—Mr. Wenham criticises Dr. Pigott's paper in the preceding number of the Journal, remarking truly that the patience of microscopists must eventually become exhausted by the repetition of the same theme. He then shows that Dr. Pigott claims, without foundation, discoveries with regard to the improvement of object-glasses and the "colour test."—Dr. Maddox, On the apparent relation of nerve to connective-tissue corpuscles, &c., in the Frog-Tadpole's Tail, describes, in connection with the observations of Eberth, Kuhne, and Moseley, cases in which nerve-fibres seem to lose themselves in connective tissue corpuscles. His results are not very decided, and hardly tend to settle the question.—Mr. Edwin Smith describes a new substage for the microscope, and certain appliances for illumination.—The paper read before the Royal Society, by Messrs. Pode and Lankester, is given in full. Their experiments are divided into eight series, in which infusions of hay and turnip, mixed or unmixed, with cheese finely divided or in lumps, are boiled and some of them sealed. When the cheese was finely comminuted, Bacteria did not appear; when in lumps, they were frequently found. In a boiled turnip infusion, placed in a retort of which the end was left open, there was no cloud developed after many weeks, which is quite contrary to the observations of Dr. Bastian (NATURE, Feb. 6, p. 275.)

THE *Geological Magazine* contains Prof. T. Sterry Hunt's article from the *Canadian Naturalist*, on the history of the names Cambrian and Silurian in Geology. The subject is divided into three parts: 1. The history of Silurian and Upper Cambrian in Great Britain from 1831 to 1854. 2. That of the still more ancient rocks in Scandinavia, Bohemia, and Great Britain up to the present time. 3. The history of the Lower Palæozoic rocks in North America.—Mr. E. Hardman describes and gives analyses of the Siliceous Nodular Brown Hæmatite (Göthite) in the Carboniferous Limestone Beds near Cookstown, Co. Tyrone. The ore contains as much as 52·2 per cent. of iron, on the average, and no sulphur.—Mr. J. C. Mansel-Pleydell has a brief memoir on the geology of Dorsetshire, which is an interesting summary of the most important points in the unbroken series from the Liassic to the Quaternary formations found in the county.—Mr. Joshua Wilson, in endeavouring to arrive at the time when the Gulf Stream reached the British Coast and so dispersed the then abundant glaciers, ingeniously shows that a raised beach, containing Arctic shells, mentioned by Geikie in his "Scenery of Scotland," must have been produced before that event, otherwise i would have been removed by the *offshore* under-current which always accompanies an onshore wind.—Dr. Winkler's description of *Pterodactylus micronyx* in the Jeyler Museum, from the Lithographic Stone of Eichstätt, in Bavaria. The specimen is very small and complete. There are four phalanges in the long finger of the hand. In the foot there are two in hallux; three on the second and third; and two, with no metatarsus, on the fourth (Stümmel).—In a letter to the editor, Mr. T. W. Danby, after comparing the new method of writing crystallographic formulæ proposed by Mr. Rutley, shows that it is not so advantageous as that of Dr. Whewell, modified by Prof. W. H. Miller; it is therefore doubtful whether its partial acceptance will not place a further obstacle in the student's path.

THE numbers of the *Journal of Botany* for August, September, and October, fully maintain the character of this magazine. In addition to the short notes and queries in each number, which often contain points of great interest to the systematic or physiological botanist, the following articles may be mentioned as of special value:—Dr. Alfred Nathorst, of the Geological Survey of Sweden, contributes a paper on the Distribution of Arctic plants during the Post-Glacial Epoch, which he considers to exhibit gradual changes of climate from the Forest-bed down to the Boulder-clay.—Prof. Church gives an analysis of the giant puff-ball, *Lycoperdon giganteum*, which he finds to contain, when dried, as much as 66·78 per cent. of albuminoids, and the ash 46·19 per cent. of phosphorus pentoxide.—Mr. J. G. Baker describes a very interesting new genus of ferns, *Diplora*, of the tribe *Aspleniæ*, from the Solomon Islands.—From the same botanist we have a valuable synopsis of the East Indian species of *Dracæna* and *Cordyline*.—Mr. J. Ball commences a description of some of the new species, sub-species, and varieties of plants collected by Dr. Hooker and himself in Morocco in 1871; the flora belongs essentially to the Mediterranean type, and the number of novelties is not comparatively large.—Mr. Carruthers gives his very valuable annual Review of the Contributions to Fossil Botany published in Britain in 1872, comprising 23 distinct papers or abstracts.—In these numbers we have also parts vi. and vii. of the Rev. E. O'Meara's Recent Researches in the Diatomaceæ.

THE second part of vol. xxix. of the *Transactions of the Linnæan Society*, just published, is occupied by a continuation of Colonel Grant and Prof. Oliver's "Botany of the Speke and Grant Expedition." The number of new species described in this part is thirty-five; and it is illustrated by thirty-five full-sized 4to plates, the expense of which is munificently borne by Col. Grant.

Der Naturforscher, August.—The eruption of Vesuvius last year attracted much scientific observation, and we have in the present serial an abstract of a valuable paper by M. Heim on the nature and formation of lava, of which he distinguishes two kinds, "lump" lava and "cake" lava (*Schollen* and *Fladen*), differing, he found, not in chemical constitution, but merely in vapour-contents. In the physical division we may note M. Wiedemann's experiments in measuring the elliptical polarisation from reflection on bodies with surface colours, for a series of angles of incidence, and different parts of the spectrum. Meteorology is represented by M. Dufour's recent observations on reflection of solar heat from the Lake of Geneva; and an interesting paper entitled "Polar Lights and Earth Lights." There is a description of M. Zöllner's new mode of estimating the absolute temperature of the sun, which is based simply on a knowledge of the density relation between two different layers of the hydrogen atmosphere, the distance between them being known. The value his formula gives is 61350°. Among botanical subjects treated are, autumn colouring of leaves and formation of vegetable acids, summer dryness of our trees and shrubs, and passage of radiant heat through leaves. Some physiological experiments by M. Rosenthal, on the time-relations of reflex phenomena, are described; and there is a variety of other matter, much of which has already been noticed in these columns.

Annalen der Chemie und Pharmacie. Band. clxviii. Heft. 1, July 16.—The number opens with four papers by Prof. Ad. Claus, on azophenylen, on di-iodhydrin, on the action of ammonia on dichlohydrin, and on the preparation of dichlorhydrin. The first of these contains a long and exhaustive account of the body in ques-

tion and of its compounds. The formula of azophenylen is $C_{12}H_8N_2$. By the action of ammonia on it a body having the formula $C_{12}H_{10}N_2$ is produced.—On diiodhydrin, by the same author. This body has the formula $C_3H_6I_2O$.—On the action of ammonia on dichlorhydrin, by the same. The result of the action is the production of chlorhydrinimid, a body of the formula $C_{12}H_{27}N_3Cl_2O_4$. —Preparation of dichlorhydrin, by the same. The method consists in acting on glycerin with chloride of sulphur.—Application of the periodic law to the cerium group, by D. Mendelejeff. —On the preparation of ethylen and its bromide, by E. Erlenmeyer and H. Bunte.—On the action of nascent hydrogen on the oil of bitter almonds, by Hugo Amann.—On the bromised benzol sulpho acids, by A. Woelz. The author has prepared dibrombenzol sulpho-acid, and gives an account of its salts and of its reaction with fused potassic hydrate.—An investigation of piperin and its products of decomposition, piperic acid and piperidin, by R. Fittg and I. Remsen.—On ethylen-protocatechuic acid by the same author, and T. Macalpine.—New compound of the Naphthalin group, by J. P. Battershall.—On the action of a mineral sulphur water on cast-iron, by Dr. E. Priwoznik. The author found an iron water-pipe, through which this water passed converted as regards its inner side into a mixture of sulphide of iron, hydrated oxide of iron and free sulphur. The centre stratum was also altered, containing only 79·2 per cent. of iron.—On sulph-hydantoin (glycolyl-sulpho-urea) by R. Maly.—Determination of boiling points at the normal barometric pressure, by Dr. H. Bunte.—Preparation of trimethyl-carbinol, by Linnemann's method, by A. Butlerow.

SOCIETIES AND ACADEMIES

LONDON

Royal Microscopical Society.—The opening meeting of the session was held at King's College, Oct. 1, C. Brooke, F.R.S., president, in the chair.—The secretary read a paper by Dr. Maddox descriptive of an organism found in a pond of fresh water in the New Forest, near Lyndhurst, which it was proposed to name *Pseudo-amæba violacea*. The general appearance of the organism was minutely described and figured, and the results of a series of continuous observations upon a growing slide under the microscope were detailed.—A paper by Mr. F. Kitton, of Norwich, describing some new species of Diatoms, was taken as read, and the attention of the meeting was called by the president to one of great beauty named by Mr. Kitto *Aulacodiscus superbus*.—Mr. F. H. Wenham made some interesting observations upon the microscopical appearance of glass which had been subjected to the action of the American sand-blast process, showing that the erosion of the surface was entirely due to the percussive force of the particles of sand, and that the results of this were demonstrated by the polariscope. A number of specimens were exhibited in the room.—Mr. C. Stewart, the hon. sec., exhibited under the microscope, and minutely described, a beautiful preparation of the spermatophores of the common squid; he also explained and illustrated the general structure of the generative organs of the male cuttle-fish.

PHILADELPHIA

Academy of Natural Sciences, April 3.—*Conchological Section.*—Dr. W. S. W. Ruschenberger, in the chair.—Dr. F. A. Hassler presented the following memorandum of experiments by W. M. Gabb and himself to ascertain the tenacity of life in *Littorina muricata*. The specimens, 140 in number, were collected by Mr. Gabb in St. Domingo, September 1870, and hung in a basket in his office. A few (five or six) were moistened after three months, then each month until May 1871, when all were alive. May, June, July, and August, 1871, 25 were moistened each month, and all found to be living except two in July and two in August. These were each month laid aside and not moistened again until September. At this time 40 of the original lot remained, all were moistened, and 29 found to be alive. In September, of the 100 which had been moistened during May, June, July, and August, 89 were alive. The 118 living ones were all placed together Feb. 18, 1872, the lot was again moistened and about 60 revived at once, and after several hours all but 24 were or had been crawling. These 24 were rejected. March 30, 1872: of the remaining 94, ten were moistened, nine were alive; these nine were placed aside with a few which had given evidence of life since the last experiment, Feb. 18. Sept. 18, 1872: all moistened and found living; they were also all alive in December. On Feb. 12, 1873, two found to be dead, and were separated from the others. March 26: All moistened, and though exposed for three days, only one began to crawl; this one was separated, also 27 others which were known to be dead, leaving 65 undetermined.

PARIS

Academy of Sciences, Sept. 29.—M. Bertrand in the chair. —The following papers were read:—Notes on the yellow elastic tissue, and remarks on its history in relation to a memoir by M. Bouillaud, and some criticisms on it by M. Bouley, by M. E. Chevreul.—Researches on the elastic tissue of the elephant and the ox, by M. Chevreul.—New researches on the analysis and theory of the pulse, by M. Bouillaud. The author continued his former papers on this subject, dealing with the abnormal pulse in this paper.—Remarks on M. Bouillaud's late paper on the pulse, by M. Bouley.—Reply to M. Bouley by M. Bouillaud. —Remarks on No. 21 of the "Mémorial de l'officier du Génie," by General Morin. The general drew attention to many interesting notes on the late sieges of Paris, contained in this number.—Note on magnetism, by M. J. M. Gaugain. This was a fourth instalment of the author's paper.—On the part played by gases in the coagulation of albumin, by MM. E. Mathieu and V. Urbain.—On a new method of treating cholera, and probably yellow fever, by means of sub-cutaneous injections of carbolic acid and carbolate of ammonia, by M. Déclat. The author recommended drinks containing carbolic acid in doses of from thirty to forty centigrammes per day, and from four to six injections of five grammes each of carbolic acid solution (2½ per cent.). These doses are to be largely increased in severe stages of the disease.—Comparison of the *Phylloxeræ vastatrix* of galls with those of roots, by M. Max. Cornu.—On the size and variation of the sun's diameter, by S. Respighi. The author, in his letter, discussed Secchi's late observations on the same subject.—On the action of the respiratory apparatus after an opening of the thoracic wall, by MM. G. Carlet and J. Straus.—On the classification of the fish of the family of *Triglidæ*, by M. H. E. Sauvage. —Researches on the action of heat on the carbuncular virus, by M. C. Davaine.—On a deposit of *Endogenites echinatus* in the Museum (fossil vegetable collection), by M. E. Robert.—On the influence of sulphates in the producton of goitre in relation to an epidemic form of that disease in a barrack at St. Etienne, by M. Bergeret.

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ERRATA.—Vol. viii. p. 480 1st col. l. 23 from bottom, for "Kerson's" read "Henson's"; l. 14, for "Montague" read "Montagne."

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